PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

GLASGOW:
PRINTED BY BELL AND BAIN,
41 MITCHELL STREET.

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

VOL. VI.

MDCCCLXV.—MDCCCLXVIII.

PUBLISHED FOR THE SOCIETY BY

JOHN SMITH AND SON,

70 ST. VINCENT STREET, GLASGOW.

MDCCCLXVIII.

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

SIXTY-THIRD SESSION.

I.—*On a Means of Preventing Accidents in Diving Bells.* By Dr. John Taylor.

Read April 5, 1865.

A year or two since a lamentable accident occurred in a diving bell on the river Clyde, which occasioned, in an instant, and without previous warning, the loss of two lives, under circumstances which will yet be fresh in the minds of most members of this Society. The cause of this accident was the bursting of an air tube and the failure of a valve. As such tubes and valves are still in constant use, and may at any moment occasion like results, it occurred to me, when investigating the circumstances at the time, that by taking advantage of the well-known laws of pneumatics, an arrangement of the diving bell might be constructed which would be an absolute security against the recurrence of such catastrophes, and might at the same time, by giving greater safety, be the means of extending the usefulness of this important apparatus.

Before describing the nature of the proposed alteration, it may be not uninteresting to allude shortly to the various methods which have, at different times, been in use for the purposes of diving.

The mode of diving without artificial assistance, but merely by the power, acquired by practice, of holding the breath for a longer than usual period, is of great antiquity. It is still followed by the pearl-divers of Ceylon and other parts of India. Some of these persons, when trained to the art from their infancy, acquire the power of remaining under water for five or even six minutes; while the extreme time during which ordinary lungs can bear immersion is from half a minute to a minute. The diver, supplied with a mouthful of oil, with his chest fully expanded with air, plunges beneath the waves; and, having attained the depth of a few feet, emits the oil,

which rises and spreads, rendering the ruffled water smooth and transparent. He continues his course downwards, and, having reached the shells, sponges, or corals of which he is in search, returns to the surface with purple lips, blood-shot eyes, and frequently emitting a bloody saliva from his mouth, and showing other signs of the depth to which his art has enabled him to descend, but at the risk of terminating his life at every effort of his skill.

Passing by the abortive, and in many cases absurd, inventions for the purpose of assisting the diver, proposed or actually tried in more ancient times, it would seem that the earliest "diving chest," as it was called, consisting of an inverted chest or bell, ballasted and lowered over the heads of the divers, was employed in the sixteenth century for the purpose of searching for the lost treasures of the Spanish Armada, near the Island of Mull, and with partial success. In 1687 a square wooden box, bound round with iron, was used by one Phipps as a diving bell, and was successful in enabling him to recover treasure to the value of £200,000 from a Spanish ship on the coast of Hispaniola. The diver in this case remained under water till the supply of air contained in the box or bell was exhausted, when he had to return to the surface for a fresh supply. One of the greatest defects of such an apparatus arises from the shrinking of the air in the chest as it descends to greater and greater pressure, as, by the law of Mariotte, the bulk being inversely as the pressure, at a depth of 30 feet the water will have risen into half the space in the bell, at 60 feet, two-thirds, and so on.

The celebrated astronomer, Dr. Halley, in order to remove this obstacle to the practical use of the bell, somewhat more than a hundred years ago, introduced the great improvement of a pair of air barrels or casks drawn up and down between the atmosphere and the bell by means of an endless cord, the one cask being drawn down and emptied into the bell while the other was being filled above by an assistant. "In this way," says Dr. Halley, in the *Philosophical Transactions*, "I was enabled for the first time in the world's history to stand and move about on the bottom of the sea, as if on land, with my clothes on, for hours together, without any ill consequences. I could see perfectly well to read or write when the sun was shining, or when it was dark I used a candle for hours together, notwithstanding the great expanse of air necessary to maintain flame." He then says, "This I take to be an invention applicable to various uses, such as fishing for pearls, diving for coral or sponges, and the like, in far greater depths than has hitherto been thought possible; also for the fitting and placing of foundations for moles, bridges, &c., in rocky

bottoms." Our countryman Halley was thus the originator of this now important branch of under-water engineering.

The next great improver of the diving bell was Smeaton, the engineer. He substituted force pumps for Halley's air barrels, and used a cast-iron bell of 2 or 3 tons in weight, raised and lowered by a crane almost exactly in the manner at present employed. He applied it with great success to the engineering works, as proposed by Halley.

The common diving apparatus, as at present used, is of two forms—viz., the Diving Bell and the Diving Helmet.

The diving bell consists of a cast-iron box or cell, of rectangular form and about 6 feet in height, with a length of 5 feet and a breadth of 4 feet, thus giving a cubic internal capacity of 120 cubic feet. It consists of roof and sides only, and has no bottom. It is fitted interiorly with a seat for the workmen at each end, with shelves for tools, and with a hook from which the candle or lamp for illumination is suspended. It is attended by a barge capable of floating it to the scene of operations. On the outside of the roof of the bell is a link for the attachment of the chain of suspension. This chain, passing from a species of crane on the deck of the barge, and round a pulley on the roof of the bell, raises or lowers it as may be required by the workmen in the interior. Signals are made by striking on the side of the bell by a hammer, water being a good conductor of sound. One stroke signifies more air; two, stand fast; three, heave up, &c.

As the atmosphere of the interior is vitiated by the respiration of the inmates, and by the combustion of the candle, fresh air is supplied by means of a hose or flexible tube, communicating with a force pump worked by men in the barge from which the bell has been lowered.

As the vitiated air is usually at a higher temperature than the pure air supplied by the pumps, it rises to the roof of the bell, rendering it requisite that the pure air be introduced through an opening in the roof, so as to drive the vitiated air down before it. As usually constructed, the hose enters by an opening in the centre of the roof, pouring down the pure air into the interior, while the vitiated air escapes below by, so to speak, underflowing the edge or mouth of the bell.

The peculiar danger to which allusion is made in the title of the present paper arises from the tendency of the compressed air to be driven out of the bell by the water rising from below. So long as the bell maintains its erect position, and the roof and hose are tight, the interior will remain filled with air, and the water cannot enter. To guard against the regurgitation of the air during the action of the pump, the foot-valve of the pump is sufficient; and so long as the hose itself, and

the joint where it fits on to the bell, remain tight, all is safe; but if the hose or the joint should leak, the compressed air would rapidly be displaced, and the interior filled by water. To guard agaiust the effect of such leakage, a valve is placed over the opening in the roof by which the air enters. In case of leakage at any point along the whole length of the hose, or at the pumps above, this valve is the only protection to the life of the diver.

It might at first sight appear that the danger from such an accident would not be greater than what would occur from the entrance of water into a boat by a hole in the bottom, as in both cases the result would merely be the immersion of the occupants in the water; but a little consideration will show that the circumstances are very different. *1st.* The vessel in this case is inverted over the occupant, so it would be much more difficult for him to reach the surface. *2d.* The bell generally rests on the bottom, and, indeed, has its edges or mouth frequently sunk (as in the work in our own river) a foot or 15 inches into the mud, rendering escape from the interior impossible till the whole bell is lifted; and as the signalling for raising or lowering the bell is done by the strokes of a hammer on the side of the bell (an operation requiring time and deliberation on the part of the diver), it is not likely that such signals would be correctly made in the event of the air escaping and the water entering by accident, unless the accident was very slow and deliberate. *3d.* The circumstances which produce the entrance of water through a hole in a boat, and those which cause the escape of air from a diving bell, are very different. The pressure in the one case may be comparatively small, only a few inches of water, and the dense water enters slowly; but in the bell the pressure may be, and usually is, great; and the air, being an attenuated body, is forced out with rapidity, the water at the same time rising freely through the open bottom of the bell. The laws of mechanics enable us to compute the time which would elapse from the moment of the occurrence of a leakage by any rupture of the hose till the whole air would be forced out and the interior be filled with water. Take the case of a rupture of the hose at or near the surface of the water, the valve at the same time not being tight. By a well-known law applicable to the escape of fluids through openings—viz., the Theorem of Torricelli—the air will escape with the velocity which a body would acquire in falling to the earth from a height equal to the height of a column of the air sufficient to produce the pressure; or, if h be this height in feet, $v = \sqrt{2gh} = 8\sqrt{h}$, where v is the velocity in feet per second. ***First***, To find the comparative times in which water and air would be forced through the opening, if h be the height of the column

of air necessary to produce the pressure of say one atmosphere within the bell in excess of the external atmosphere, or two atmospheres in all, which would be the case at a depth of 30 feet; then, if h_w denote the height of a column of water capable of producing the pressure, 400 h_w would be the height due to air of double the atmospheric density. The ratio of the density of air at common pressure to water being as 1 to 800, the velocities will be in the proportion

$$\begin{array}{rcl} \sqrt{h_w} & : & \sqrt{h} \\ \sqrt{30} & : & \sqrt{30 \times 400} \\ \sqrt{30} & : & \sqrt{12000} \\ 5 & : & 109 \\ 1 & : & 20 \text{ nearly.} \end{array}$$

So the bell would empty itself of air in one-twentieth of the time that water under similar circumstances would require to pass through the same size of opening.

Second, To get the absolute time of filling. As $2\frac{1}{2}$ miles is the height of a column of air, of double the ordinary density, necessary to give a pressure of two atmospheres,

$$\text{we have} \quad \begin{aligned} v &= 8 \sqrt{2\tfrac{1}{2} \text{ miles.}} \\ &= 8 \sqrt{13200 \text{ feet.}} \\ &= 8 \times 115 \\ &= 920 \text{ feet per second, or} \\ & \quad 11040 \text{ inches per second;} \end{aligned}$$

which, if through an aperture of *one square inch*, would give 11,040 cubic inches per second, or 1 cubic foot in a sixth of a second, or 6 cubic feet per second.

If the bell be 7 feet high, 5 feet long, and 4 broad, or 140 cubic feet, 23 seconds would empty it of air or fill it with water. If the opening be less or greater than a square inch, a correspondingly less or greater time would be taken. This shows that even a very small opening would cause great danger; and that in the event of the supply of air ceasing from above, nothing short of absolute tightness in every part of the roof could be consistent with the safety of the occupants of the bell, for even a few seconds. Now, even after alarm was taken of the supply having ceased and escape having begun, a signal has to be made, and the operation of lifting gone through with the double chain and ponderous movements of the crane. Even if the bell were lifted from the ground, on its progress upwards, before it filled with water, the probability is that the men would, in the struggle of drowning, fall from their seats, and sink through the open bottom, and be lost, as seems to have happened with the two men in the late

accident on our own river already referred to. The bell came up empty, and even the bodies were not found.

If the rupture of the hose occurred at a point at some depth under water, the velocity of escape of the air would be less, and would be found, by using the pressure due to the difference of depth between the point of rupture and the position of the bell itself, as the effective "dynamic head" in the above calculations.

When it is thus remembered how readily air, under pressure, escapes even by small openings, and also how the best fitting valve may be rendered useless by a slight bend of its spindle, by a particle of dust, verdigris, a bit of decayed leather from the interior of the hose, by the breaking of the spring which keeps it in its place, or by the mere wear of continued action, it will readily be granted that any arrangement upon which depends the safety or the instant destruction of human life ought not, if possible, be made to depend on the accurate fitting of a valve. As to the bursting of the hose by a pressure such as is necessary to send air to a depth of 30 or 40 feet under water, such an accident will seem far from unlikely if we consider the circumstances. Say that the depth is only 30 feet, a pressure of 15 lbs. on every square inch will tend to burst the pipe. Now, suppose that the internal diameter is 2 inches, the force tending to tear asunder every longitudinal inch will be 30 lbs. It is as if a ring, an inch in breadth, of the hose were taken and put over a hook, and 60 lbs. weight hung from it.

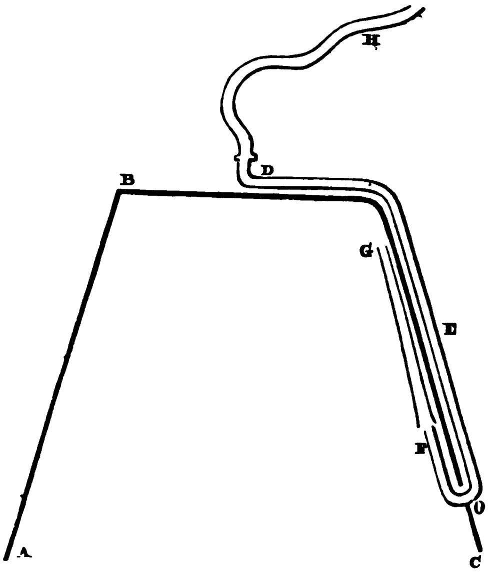
No doubt, in the arrangements made for safety, these tubes and valves are ordered to be frequently tested; but such testing requires an amount of care and skill not likely to be always at hand when wanted.

Shortly after the accident referred to, I proposed to the Clyde Trustees the adoption of a mode which would dispense with the use of an opening in the roof of the bell, and would thus get rid of all danger, there being no valve required.

The method consists in using a bell without any opening in the roof, and in attaching the hose to a metal pipe which descends from the roof *outside* the bell to a point at or near the *bottom*, where it enters and turns up, and delivers the air to the interior. As this air is to be delivered at the roof, that it may displace the heated and vitiated atmosphere surrounding the heads of the divers, it is requisite that the tube be carried upwards to the roof. Now, it is evident that a valve in this case would be required, as the open pipe would still afford a channel of communication between the outer air and the compressed air of the bell. But if an opening is made in the tube at any lower level of the bell, say at the height of a foot from the bottom, then this opening may be so formed that the blast of air will not escape at it,

but will proceed up the pipe to the roof, as it ought to do; but, on the contrary, if water rise in the bell, the water will enter the opening and effectually close the tube, preventing all further escape of air and rise of water above the level of the hole in the tube. Instead of a mere hole being made, to give greater security the tube may be interrupted at that point and drawn to a nozzle, which may then blow the air into the funnel mouth of another tube, which conducts it to the roof. The opening might be made at the lower part or bend of the tube, in which case the water would not rise at all in the bell; but in case of the bell sinking its edge into the mud, the opening in the tube might be closed, and an accident result. This objection was at first urged by the Clyde Trustees against the plan; but on its being pointed out to them that, by making the opening a foot or 15 inches up the tube, no such filling up by mud could happen, they adopted the plan in the bell in which the two men had lost their lives, and it has been in constant action since.

The annexed sketch represents in section the proposed arrangement:—A, B, C is the bell; D, E, O, F shows the metal tube descending on the outside and entering the bell by the opening O, where it



turns up, and terminates in a nozzle at F, and it delivers the air to the funnel-shaped mouth of the larger tube F G, by which it is conducted to the roof of the bell at G; H is the hose for the supply of air from the pumps.

In estimating the proposed arrangement it ought also to be borne in mind that it is simple, and can be adopted in any existing bell. Valves can be used along with it if thought desirable. If no accident shall ever bring it into action, still it is there, and causes no inconvenience; and if a burst ever shall occur, the water could not rise higher than perhaps the knees of the divers, while air enough will remain to support life for a considerable period. Allowing that a cubic foot will support the life of one person for eight minutes, and that there are two in the bell, each cubic foot will last four minutes. If there is 4 feet of depth of air in the bell, it may, in case of extremity, support life for two or three hours.

With reference to the mechanical principles involved, it may be remarked that the force urging the water into every part of the bell is the hydrostatic pressure due to the depth. This is also the force urging the air into the tube at G, and tending to keep the water from entering the tube at F. So the water, when it rises to F, is forced in merely by seeking its level, and fills the tube, and thus seals it; all above this point of the bell being without opening, the security is the same as if there were *no opening.*

It may be mentioned that, in the case of the bell on the Clyde, the men not being able to trust the principle thoroughly, have made the experiment in comparatively shallow water by detaching the hose from the pump, and in every instance with perfect security.

The diving helmet is similar in construction to the diving bell, with the exception that, as it is intended only to afford breathing space to one person, while it is to allow of the free use of the limbs in walking about and working under water, it is made only to envelop the head and neck, leaving the arms free. To enable the diver to see objects outside his helmet, large glass eyes are inserted in its sides. It is supplied with air by means of a flexible hose, in communication with a force pump above water, as in the case of the bell.

The diving helmet is virtually the invention of Halley, the astronomer. His form of it was intended to enable one of the divers to pass out of the main bell, and to go into recesses under ships' bottoms, and into other localities which the bell itself, owing to its size, could not approach.

A small cell, like an inverted basin, was placed over the diver's head, and had a flexible air tube attached to it, terminating in the chamber of the main bell. So long as the small cell was kept above the water-level of the floor of the main bell, air would rise into it from the main bell, the pressure being less. The diving helmet at present in use differs from this chiefly in the circumstance that air is supplied

to it by a hose from a force pump worked on the surface of the water. In fact, it is only a small diving bell, with windows in its side; so that all the remarks made with regard to the dangers of the whole of the air escaping from the interior of the bell apply in this case. Exactly the same arrangements, as above described, can be applied with the certainty of saving a cubic foot of air, even in case of a rupture of the hose. A cubic foot of air will, on an emergency, support the breathing of a man for six or eight minutes, a period which would give time for rescue.

II.—*On an Electric Apparatus for Detecting the Presence of Pieces of Metal, such as Musket Balls, &c., in Gunshot Wounds.* By DR. JOHN TAYLOR.

Read April 5, 1865.

IT will be in the recollection of many members of the Society that, in one of the late actions in the revolutionary war in Italy, the famous General Garibaldi was wounded in the ankle by a musket bullet; and also, that a long and painful illness resulted. The ankle joint was not seriously affected, yet the wound refused to heal. Many examinations were made, in order, if possible, to ascertain whether the bullet was imbedded in the bone; and much difference of opinion as to the point prevailed among the Italian surgeons. The most experienced surgical assistance was procured, both from France and England. The wound was enlarged, probes and the fingers were introduced without giving any satisfactory information. Dr. Partridge, from London, confidently asserted that the bullet was not in the wound, and recommended treatment accordingly, but without benefit. A French surgeon at last recommended a piece of rough porcelain to be introduced into the wound, and moved about as a file, in order that if lead were present, it might, by abrasion, remove a portion of it. This painful expedient succeeded; portions of lead adhered to the porcelain. The bullet was in the wound, but as its form could not be seen, it could not be grasped with forceps, as it might be mistaken for bone, or bone mistaken for the bullet, and injury done to the joint. The wound was enlarged, and the slow process of suppuration trusted to for dislodging the offending body. The delay, as it happened, was successful. The bullet fell out of the wound, fortunately before the strength of the patient had been thoroughly exhausted. The

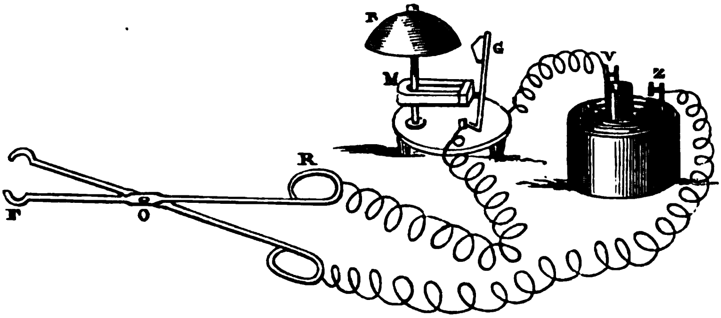
recovery was excessively slow, from the irritation resulting from a long illness. There cannot be a doubt that, had there been a means of *at once* detecting and extracting this bullet, a great amount of suffering would have been prevented. It is evident, also, that had the foreign body enclosed in the wound been iron, such as the fragment of a shell, instead of a soft metal like lead (as it happened to be), the porcelain probe would not have been successful in indicating its presence.

It occurred to me at the time, but unfortunately too late to be of service to the patient in the case referred to, that a well-known property of electricity might be turned to account in such cases. Metals are good conductors, whereas bone and the other substances of the animal body are indifferent or bad conductors. We have only to make the body in question part of an electric circuit, and to apply any test to show whether the circuit is completed, or, in other words, whether the body is a conductor. For this purpose it is not requisite that the body be seen, but merely that it be touched. In this way the electric probe, as it may be called, was constructed.

It consists of two wires, covered separately with some non-conducting material, such as silk, throughout their whole length, with the exception of a portion near the points, where a small interval is left between them. The other ends of the wires are made to communicate with a galvanometer, or with an electro-magnet and a bell, and with a galvanic cell, in such a way as to form a circuit complete at every part, except at the space between the two points of the probe. If these two points are then made to touch any conducting body, such as a piece of lead, the circuit is completed, and the galvanometer or the electro-magnet give corresponding indications. So the probe has only to be moved about in the interior of the wound, and the galvanometer will indicate the instant it touches the metal, or the bell will ring if the electro-magnet be used.

But even after the presence of the foreign body is indicated, how is it to be extracted? The process by the porcelain probe indicated the presence of the bullet to Garibaldi's surgeons, but it did not enable them at once to extract it. Neither will the electric probe; but if we insulate the two blades of a pair of forceps, by means of a non-conducting joint, they will themselves serve at once for a probe and also as an indicator of the *kind* of body, whether a piece of bone or a bullet, which is within their grasp. Nothing more is required: we have only, with a suitably shaped pair of forceps, arranged as part of an electric circuit, to feel about the wound, gently grasping one part after another, and having got the metal, to extract it with the utmost confidence that it can be nothing else than the object we are in search of.

The annexed sketch represents the arrangement:—F, O, R shows the forceps, the one blade being electrically insulated from the other at O by means of ivory, vulcanite, or other suitable material. M, B, G is the



electromagnetic bell, while V Z is a voltaic cell communicating by wires with the electro-magnet and with one blade of the forceps, the other blade of the forceps being similarly united to the remaining wire of the electro-magnet.

It is only just to state that after going thus far with the apparatus for detecting and extracting metallic bodies in the manner now described, I found, from a report in the *Comptes Rendus*, that a surgeon of Marseilles had invented exactly the same kind of probe, and described it about the same time in a communication to the Academy of Sciences of Paris. He had not, however, done anything with regard to the mode of extracting the body. So I think it is doing no one any injustice to say that this is the first pair of forceps sensitized by an electric current for the purpose of extracting metallic bodies in the manner described.

A case which came under my own observation in Edinburgh many years ago will well illustrate the use to which such an apparatus as the one described may be applied. A servant girl presented herself at the Edinburgh Royal Infirmary, asserting that she had swallowed a padlock. She and a fellow-servant had been fighting for possession of the lock; she put it into her mouth, and in the struggle swallowed it. Pointing to her neck, she said, "It is there; I feel it sticking in my throat." Mr. Syme, the well-known surgeon, carefully examined the case, exploring the throat with a probe, and, after several days' delay, came to the conclusion that it was either a case of feigning or of mistake, and dismissed the patient.

Mr. Lizars, hearing of the case after the girl had left the hospital, had her recalled and carefully examined. Something hard was reached in the throat by the probe, but the nature of the foreign

body, or its situation, could not be ascertained with sufficient certainty to enable it to be pulled out by the forceps. After a delay of a number of days, symptoms of irritation and fever having set in, it was at last determined to open the throat by an incision in the neck, and to examine by absolute inspection whether the girl's statement was a true one. The time for the operation had been fixed; but on the previous night the patient was accidentally seized with sickness and retching, and the house surgeon having been called, he found that the padlock had been brought up to within reach of the forceps, and admitted of easy extraction.

What I assert is, that had there been in such a case a certain method of distinguishing, by the touch of an instrument, a metallic body, there would have been no cause for embarrassment to the surgeon, nor of danger to the patient's life. The sensitized forceps would at once have enabled the operator to grasp the body, and, fearlessly, to extract it.

There is little doubt but that other applications of this method of discovering the position of pieces of metal which cannot be seen, may be made. Iron cables and anchors may be searched for by a drag traversed by an electric current. The exact position of a sunken iron ship or boat might be ascertained by a sounding lead fitted with two insulated points, stronger, but similar to the probe.

III.—*Description of a Heat-Restoring Gas Furnace, constructed for Heating Iron, according to the Patent of William Gorman and John Paton, with Results and Observations.* By Mr. William Gorman.

Read April 19, 1865.

The Heat-restoring Gas Furnace was constructed at Govan Bar Iron Works early in the year 1864, and has received constant attention ever since.

The furnace is similar in arrangement to the ordinary balling or re-heating furnace, and occupies about the same space. It differs only in the mode of generating the heat, and in having an apparatus, which we call a restorer, added for returning the waste heat.

In the ordinary furnace the coke or solid part of the coal is completely burned on the grate, and in consequence, as explained in former papers, the combustion of the gaseous part of the coal is prevented, and of course the heat due to the coal gas is lost.

In the gas furnace the solid part of the coal is converted into carbonic oxide gas, which is combustible, and, along with the coal gas, is

burned with a further supply of air in the part of the furnace where the heat is wanted. By this process the heat from the gas, as well as from the solid part of the coal, is obtained.

The chamber where the coal is converted into gas occupies the place of the fire and grate of the ordinary furnace, and it is made much deeper, in order to insure that no carbonic acid gas be allowed to escape amongst the combustible gases.

The furnace may be used either with or without a grate. When a grate is used, it must be constructed so that it can be cleaned while the furnace is working. The bars are horizontal, supported on bearers nearly a third of length of the bar from the ends. The bars are free at each end, with an open space above, through which ashes or clinker can be drawn; the bars have room to slide along nearly a third of their length, and can be drawn out or replaced when wanted. We have found this arrangement to answer very satisfactorily.

The producer is wrought with close doors, which are cast thin, with fins projecting outwardly to keep them cool, after the manner of the Gill Stove. Previously the doors were lined with brick; but we find this arrangement answers better: it does not heat much, and it is not likely to go wrong.

When the gas from the coal leaves the producer, carrying with it the heat generated there, it is supplied with air for its complete combustion, which in this instance it was found most convenient to introduce at the bridge, previous to its entering the heating chamber of the furnace, when it produces a most intense heat, sufficient, if required, for completely melting malleable iron, which, indeed, it has done several times, and in large quantities.

The air for the partial combustion of the solid coal, and expelling the coal gas, is supplied cold; but that for burning the gases is heated to a very high temperature by the waste heat of the furnace. We attempted, but have not yet been successful in working the gas producer with heated air, but expect, with suitable arrangements, to effect this object also, which should still further economize fuel.

The air required for combustion is found, in practice, to be about twenty-five times the weight of the fuel. It becomes, therefore, a convenient vehicle for returning the heat, which usually goes to waste. The more heat which can be imparted to the air supplied for combustion, the less coal is required to maintain a given temperature in the furnace. The question then becomes, What is the best practical method of transferring the greatest amount of heat from the highly heated waste products leaving a furnace, to air entering a furnace for combustion?

The apparatus we employ for this purpose, called a heat-restorer, is based on a very elegant instrument, to which my attention was first called, about the year 1844, by the late Mr. Condie, whose success in overcoming the practical difficulties attending the introduction of the hot-blast are so well known. The instrument is for transferring heat, and consists of two tubes, one placed within the other,—the inlet of the one tube adjoining the outlet of the other. Of course, the tubes are open at both ends. If hot water be poured in one tube, and cold water in the other, the hot water will run out cold, and the cold water will leave the instrument heated very nearly to the temperature of the water which was poured in hot. The waste heat is transferred to the air for combustion by the restorer in the same manner. The air is introduced at the point farthest from the furnace, and gets heated as it advances towards it, where the gases are hottest; and conversely, the hot waste gases, as they proceed towards the chimney, meet the air at its greatest heat first, and the temperature gets lower and lower as they proceed towards the chimney; by exposing great surface, and continuing the process, the heat can be effectually restored to the furnace by heating the air.

Mr. Stirling's Patent of 1816 includes heating air for furnaces by passing it through a mass of brickwork which had been previously heated by passing the flame, which usually escapes, through amongst them in an opposite direction. Two chambers of brick-work were required,—the one to take up the heat from the escaping flame, whilst the other was heating the air or blast; and *vice versâ*, the waste flame passing out through those chambers alternately, and the cold air passing towards the furnace through those chambers in an opposite direction, and at the intervening times. Mr. Stirling subsequently applied this process to stoves, and to recovering the heat from waste hot water at dye-works, &c. Stirling's air engine, Siemen's regenerative condenser and gas furnace, and Erricson's air engine, have regenerators of this kind.

The different apparatus of this kind are usually called regenerators; but they do not regenerate, they only transfer heat; and they are now referred to, to show that their apparent simplicity and utility do not hold good in practice.

In the first place, it is necessary to have two separate regenerators, and double sets of valves, for reversing the currents of the air and flame or hot gases—(those valves require to be worked by hand, and if they are neglected, they get burned), and double sets of passages, which render the apparatus complicated and liable to go wrong.

Secondly, as to utility. Suppose the mass of brickwork composing

the regenerator heated at one end by the escaping hot gases to a temperature of 4,000°. When the current is reversed, the temperature is gradually reduced by the air which is being heated, till it is reduced, say to 2,000°, before the currents are again reversed. The mean temperature obtained is obviously only 3,000°; and, in order to obtain this temperature of 3,000°, the brickwork of the furnace, &c., must endure a temperature of 4,000°. The furnace, and iron or steel, &c., which are being heated, are thus subjected to an unnecessarily high and fluctuating temperature, the evil effects of which, and the danger of burning or injuring the iron or steel, are well known to practical workmen. But, independently of those obvious objections to this kind of regenerator, it is not calculated theoretically to communicate the maximum heat to the air, as it must necessarily return only the mean between the different degrees of temperature at which the currents are reversed.

The regenerator, or rather the restorer which we employ, obviates the above objections in a very marked degree. There is only one restorer; and, as there is no reversing of the currents, valves are not required for that purpose, and the heat of the furnace advances steadily from the lowest to the highest temperature; so that only culpable negligence on the part of the heater would burn the iron or steel, or the material being heated; and what the furnace gets it retains, and does not require to be subjected to a heat higher than what is just necessary to do the work.

The restorer is a chamber placed under the ground line, into which is placed a number of fire-clay pipes open at each end. A wall runs up at each end of the pipes, dividing the chamber into three compartments, one large in the centre, and one at each end of the pipes, into which they open, connecting the smaller side chambers. The flame or waste heat from the furnace passes downwards through the centre compartment, impinging on the outside of the tubes placed therein. The air for combustion enters the side space at the bottom, passes through the pipes to the other side, rising to a higher series of tubes, and re-crossing, till it arrives at the top of the chamber, the effect being an upward current of air meeting a downward current of heated gases, with only the thickness of the clay tube (less than an inch) between them; the current of air inside preventing the destruction of the tube by the high temperature outside. The only extra about this furnace which has no counterpart in the common furnace, is the restorer, and in practice it gives us no trouble. We have had the pipes taken out, from causes incidental to all new trials; but of late it gives us no concern whatever, as it is quite evident, when looking into it, that there

is no heat to damage brickwork. We have brought some of the pipes which were in use when the furnace was frequently so hot as to melt malleable iron, to show that they are not in the least impaired, and have not been subjected to anything like a destructive heat.

We believe the restorer to be a useful, practical, and durable apparatus. As to its economy we cannot give the exact figures; but from practical observation we believe that, with a given quantity of coal, the furnace will heat three times the quantity of iron which is heated in the ordinary furnaces, and with coal of an inferior quality. We also intend to work the furnace with common dross.

We expected also to have been able to show a considerable saving in iron, as it is very evident, from the sharp appearance of the piles when they are heated, that there is very little oxydation. Owing to the strike amongst the puddlers, this matter has been deferred; but, from former observations, the saving in iron will scarcely be less than 5 to 7½ per cent. on the whole iron charged.

Iron does not burn or waste in the improved furnace as it does in the ordinary furnace. We have brought some pieces of iron which were burned in the ordinary furnace, and also some pieces which were melted down in the gas furnace. The effect of the highly oxydizing flame of the ordinary furnace in fraying the iron away into cinder is very obvious from the rotten, porous coating of the one; and the effect of the non-oxydizing flame of the gas furnace is also clearly shown by the smooth, solid surface of the other, although its highly crystalline fracture evidences that it has been completely melted, and consequently has been at a far higher temperature than the burned iron from the ordinary furnace.

The brickwork of the gas furnace also shows that the iron is not much oxydized, as the oxide of iron is far more destructive on the brickwork than the mere heat of the furnace, and the bricks are not cut away in a week or two, as in some parts of the ordinary furnaces.

It is also believed that iron will improve by being heated in the gas furnace, as it is always surrounded with a non-oxydizing flame; and as there is always a plenum of pressure inside the furnace, there is no danger from the iron being cut away by draughts of cold air, and consequently no need for building solid coal in the charging door of the furnace.

The general practice of building coal in the door of a re-heating furnace, after it is charged with iron, requires a few remarks; but in order to understand the importance of the consequences attaching to the practice, it is necessary to take a glance at the history of the manufacture of iron.

Previous to the introduction of coal, the refining of iron was all done with wood charcoal, the iron being worked in contact with the solid charcoal; but it was found that iron refined with solid coal or coke in the same manner was rendered useless, its strength and tenacity being destroyed by the sulphur and other impurities in the coal or coke; and it was only after many attempts and failures that it was found that iron heated with the flame of coal was not liable to be injured by the impurities contained in the coal. Hence the process of refining iron by puddling in a reverberatory furnace; and when solid coal is laid in contact with iron at a high temperature, as in the practice referred to, the same evils are produced which puddling was introduced to remedy.

Besides, when iron is highly heated in contact with solid coal or coke, it is re-converted back into some of the forms of steel, or perhaps cast iron. I have not made any direct experiments to prove this statement; but believe it will be recognized by metallurgists as being an explanation of a fact with which engineers, shipbuilders, and boiler-makers are well acquainted—viz., a sound-looking plate, when punched or sheared, breaking away at some part of the edge or end as short as cast iron.

If the iron were properly puddled, this could not occur in the gas furnace; but it may occur at any time, and must often occur in the ordinary furnace.

The gas producer was placed below the furnace at first, and produced very good results; but it was desirable that the usual furnace arrangements should be adhered to as much as possible, and the gas producer was built on the same level as the rest of the furnace; and in order to prevent cold air from getting into the furnace, it required to be worked under pressure, for which purpose a small steam fan was employed. It gives complete control over the furnace in every respect, both as to heating rapidly and the quality and quantity of flame.

The furnace is also admirably adapted for puddling, and melting cast iron for foundry purposes. The iron will not be deteriorated, as in the cupola, and dross can be used instead of coke. It is also well adapted for large forgings and ordinary smithy purposes. In short, for working iron or steel in every way it is far better than any mode of heating by solid coal, as flame, although produced from inferior coal, or even dross, is superior to the finest solid coal for working or welding iron or steel.

I have great pleasure in stating that every facility was granted, and no expense spared by the firm, in bringing the furnace to the greatest perfection; and the Engineer, my colleague in this Patent, has applied his practical skill and well-known ability with untiring energy to the

same end. We are now in a position to recommend the furnace, and guarantee its results as to economy in fuel; and we believe the improvement in the quality, and the economy in working iron, will be far more important than the established saving in fuel.

The accompanying plate is a longitudinal sectional view of the Heat-restoring Gas Furnace, as applied to heating piles for a Plate Mill.

A is the door for supplying coal into the gas producer B, the ashpit of which is closed; the air for partial combustion being supplied at C. The gas, in passing over the bridge, is supplied with highly heated air at D, through the passage E, leading from the restorer. The combined gas and hot air produce intense heat in the chamber F, where the piles to be heated are placed. After having acted on the piles, the escape gases leave the heating chamber at G, and enter the restorer at H, and passing down amongst the tubes, are deprived of their heat before entering the passage leading to the chimney.

The air for burning the gas is passed through the first two rows of pipes at the bottom, and, rising, returns through the second three rows of pipes, thence, rising higher, through the top four rows, where it enters the passage leading to the combustion or heating chamber of the furnace, having attained a very high degree of temperature.

Since the above paper was read, we have ascertained, from returns of trials, the exact yield of the Gas Furnace, as compared with furnaces of the ordinary kind. The following are the trials referred to:—

Five piles, each 470 lbs. = 2,350 lbs., were weighed for the purpose, and charged into the Gas Furnace. At the same time, the same number of piles of equal weight were charged into the ordinary furnace.

The Gas Furnace produced, from 2,350 lbs. of iron charged, 2,247 lbs. rolled iron, the loss being only 103 lbs., or 4·38 per cent. of the iron charged.

The ordinary furnace produced, from 2,350 lbs. iron charged, 2,058 lbs. rolled iron, the loss being 292 lbs., or 12½ per cent., nearly.

The yield, or iron required to produce a ton of rolled iron is therefore,

	Ton.	cwt.	qrs.	lbs.
With the Gas Furnace, ...	1	0	3	19
With the Ordinary Furnace, . . .	1	2	3	10.
	0	1	3	19

So that, in the Gas Furnace, 1 cwt. 3 qrs. 19 lbs. is saved, or less iron is used in producing a ton of rolled iron than with the ordinary furnace.

Those results were confirmed by subsequent trials, the returns of which are within a fraction of the above statements.

We have had the Furnace worked regularly, heating three piles, each 1,400 lbs., = 4,200 lbs. per heat; and four heats within twelve hours,—four heats of 4,200 lbs. = 16,800 lbs. = 7 tons 10 cwt., which, according to the above data, if heated in the Gas Furnace, would produce of rolled iron 7 tons 3 cwt. 2 qrs. 10 lbs.;—and 7 tons 10 cwts. heated in the ordinary furnace would produce 6 tons 11 cwt. 1 qr. 11 lbs.; consequently 12 cwt. and 27 lbs. of iron is saved by the use of the Gas Furnace in twelve hours.

MINUTES.

Anderson's University Buildings, November 2, 1864.

THE Sixty-third Session of the Philosophical Society of Glasgow was opened this evening—Professor Henry D. Rogers, the President, in the Chair.

Mr. John Gray, 150 West George Street, proposed as a member at the concluding meeting of last session, was elected.

The President presented the following Books to the Library, viz.:—

Carpenter's Zoology, 2 vols., 8vo. London, 1845.
Mantell's Medals of Creation, 2 vols., 8vo. London, 1845.
Smith's Natural History of the Human Species, 8vo. Boston, 1851.
A. K. Johnston's General Gazetteer.
The United States Sanitary Commission, 8vo. Boston, 1863.

The thanks of the Society were voted to the President for his gift.

The President delivered an opening address, in which he reviewed and commented upon the proceedings of last session of the Society.

The Secretary read the following

OBITUARY NOTICES.

Since last session, the Society has had to lament the death of two of its Honorary Members, namely, Alexander Hastie, Esq., who long held the office of Secretary, and Dr. Robert Dundas Thomson, of London, who took an active part in the business of the Society during his residence in this city.

Mr. Hastie entered the Society in the year 1827, and in the same session was appointed an Honorary Member, out of respect, it would appear, to the memory of his father, then recently deceased, and whose long services to the Society, of which he was for many years President, the minutes of the time warmly acknowledge. Mr. Alexander Hastie became Secretary in 1834, and continued to perform the duties of that office till his connection with municipal affairs rendered it necessary to devolve these duties upon an assistant, and subsequently a Joint-Secretary. He was elected to the Clyde Trust in 1835; became a member of the Town Council in 1837; was chosen Lord Provost of the city in 1846; and in 1847, was elected one of the Members of Parliament for Glasgow, from which office he retired in 1857. He was afterwards appointed a member of the Royal Commission on the Scottish Univer-

sities, in the service of which he closed a public life of great and acknowledged usefulness. Fortune had rewarded his honourable career as a merchant; and he had just entered on possession of an estate which he had acquired in Fifeshire, when he was cut off, on the 13th of August, 1864, in his fifty-ninth year.

Dr. Robert D. Thomson died at Richmond, on the 17th of August, 1864, in the fifty-fourth year of his age. He was the son of the Rev. Dr. Thomson, minister of Eccles, Berwickshire. His professional education was first prosecuted in the University of Edinburgh, and afterwards under his uncle, Dr. Thomas Thomson, Professor of Chemistry in the University of Glasgow. On completing his curriculum, he received the appointment of Assistant-Surgeon in the East India Company's Navy, and proceeded on a voyage to India and China. On his return he commenced medical practice in London, where he conducted a monthly periodical, entitled "Records of General Science," and, jointly with Dr. Farr, edited another journal, "The Annals of Medicine." On his marriage with the daughter of Dr. Thomson, of Glasgow, he removed to this city, where for a period of ten years he assisted his uncle, who had become enfeebled by age, in discharging the duties of the Chair of Chemistry. He became a member of the Philosophical Society in 1841, when Dr. Thomas Thomson was President, and from that period till his connection with the Society terminated, in 1852, was a frequent contributor to its proceedings. Volumes I. to III. of the printed *Proceedings* contain papers by him "On Parietin;" "On Supplying the Poor with Food;" "On the Cure of Blindness produced by Oil of Vitriol;" "The Analysis of Cowdie Pine Resin;" "On the Nutritive Power of Bread and Flour of different Countries;" "On White Serum;" "Analysis of Ceradia Resin;" "On Analysis of Minerals;" "On Chemistry of Food;" "On Digestion;" "On Rainfall near Glasgow;" "On Sanitary Reform;" "On Shea Butter;" "On Test for Alcohol;" "On the Vinegar Plant;" together with notices of a less formal description. While he was himself a voluminous contributor to the *Proceedings*, he was indefatigable in his exertions to obtain papers of a useful description from other members, and from men of science unconnected with the Society. The results of investigations prosecuted under his direction by students in the University Laboratory were in this way brought under the notice of the Society. He edited the *Proceedings*, and, along with Mr. John Joseph Griffin, now of London, devoted much time and attention to the improvement of the Library, adding many scarce and valuable works, and completing series of publications which had been left imperfect and valueless. He was also at pains to acquire rare and curious publications connected with the history of Glasgow, such as, to recall his

own remark, "might be serviceable to any future writer on the antiquities and progress of the city." He succeeded Mr. Griffin as Librarian, and in that capacity spared no labour to enrich the collection by a judicious choice of works in which it was defective, besides presenting a succession of published statistical documents. As a member of the Society's Committee appointed to co-operate with a Committee of Town Council, he was actively engaged in the preparations for the great and successful public exhibition in the City Hall during the Christmas holidays of 1846. With a view to the diffusion of a friendly feeling amongst the members, he promoted those social re-unions which for several winters formed a pleasing feature in the intercourse of the Society. Services so varied and valuable were duly appreciated by the Society, which recorded its grateful acknowledgments to Dr. Robert Thomson, and appointed him an Honorary Member when, on the death of his distinguished relative, he left Glasgow and returned to London. He was no sooner settled in the metropolis than he was appointed Professor of Chemistry to St. Thomas's Hospital. When the Metropolitan Management Act came into operation, he was chosen to the post of Medical Officer of Health, the exacting duties now devolved upon him rendering it necessary to resign the Chemical Professorship. The reports published occasionally by the Registrar-General on the quality of the water furnished to the metropolis were founded on analyses performed gratuitously by Dr. Thomson. The subject was one which had interested him in Glasgow; and the London reports led to his being consulted as to the improvement of the water supply to Liverpool and other provincial towns of England. In addition to the works already mentioned, Dr. Thomson for three years edited a scientific publication entitled *The British Annual* Whilst in this city he conducted, at the instance of Government, a series of elaborate and costly experiments, the results of which were embodied in a volume on *Food for Cattle*. He also published an elementary book on *School Chemistry*, and an *Encyclopædia of Chemistry*, besides contributing to the *Transactions of the Royal Society*, of which he was a Fellow, and the *Meteorological Society*, of which he was President. The energy of character and restless activity indicated by these multifarious labours were united to great kindness of heart and a disposition thoroughly benevolent. And it deserves to be recorded of Dr. Robert Thomson, that in the rivalry of honourable ambition he never cherished an ungenerous feeling towards a competitor; nor in the keenest collision of opinion was he tempted to detract from the merits of an opponent.

November 16, 1864.—*The* President *in the Chair.*

The following were elected members of the Society, viz.:—

Mr. W. Newton MacCartney, 98 Hutcheson Street.
Mr. Thomas P. Miller, Springfield Works, Dalmarnock.
Mr. George Watson, 107 Union Street.

Mr. William Ramsay, the Interim Treasurer, gave in the following Abstract of the Treasurer's Accounts for Session 1863–64:

Dr.

1863.—*Nov.* 1.		
To Cash in Union Bank,	£23 0 11	
,, Do. in Treasurer's hands,	1 18 6	
		£24 19 5
,, Entry-money and Dues from 24 new Members, at 42s.,	£50 8 0	
,, Annual Dues from Four Original Members, at 5s.,	1 0 0	
Do. from One Original Member, five years,	1 5 0	
Do. from 242 Members, at 21s.,	254 2 0	
Do. from Four Members, two years,	8 8 0	
		315 3 0
,, Institution of Engineers for Rent,		15 0 0
,, Interest,		3 18 5
,, Taxes recovered from Landlord,		2 17 0
		£361 17 10

Cr.

1863.—*Oct.* 31.		
By New Books and Binding,		£84 2 5
,, Printing and Illustrating the Proceedings of the Society,		31 10 6
,, Printing Circulars, &c.,		15 10 0
,, Stationery,		2 1 2
,, Salaries, Wages, Delivery of Circulars, &c.,		120 12 10
,, Rent, Insurance, and Gas,		53 2 11
,, Petty Charges, Postages, &c., and Taxes,		11 13 1½
,, Balance at the Bank,	£28 19 0	
Do. in Treasurer's hands,	14 5 10½	
		43 4 10½
		£361 17 10

The thanks of the Society were voted to Mr. Ramsay for his services as Interim Treasurer.

Dr. Bryce made a verbal report on the Library. The number of volumes now amounts to 3,637.

The Society then proceeded to the Sixty-third Annual Election of Office-Bearers, when the following were appointed, viz:—

President.

PROFESSOR HENRY D. ROGERS, LL.D., F.R.S.

Vice-Presidents.

PROFESSOR ALLEN THOMSON, M.D., F.R.S.
DR. FRANCIS H. THOMSON.

Librarian.

JAMES BRYCE, LL.D., F.G.S.

Treasurer.

MR. JAMES REID.

Secretary.

MR. WILLIAM KEDDIE.

Council.

MR. ALEXANDER HARVEY.	DR. JOHN TAYLOR.
MR. THOMAS M'GUFFIE.	MR. GEORGE C. FOSTER, B.A.
MR. JAMES M. GALE.	MR. JAMES R. NAPIER.
PROF. W. J. M. RANKINE.	PROF. W. T. GAIRDNER, M.D.
MR. JAMES NAPIER.	PROF. ROBERT GRANT, M.A.
MR. ROBERT HART.	REV. HENRY W. CROSSKEY.

On the motion of MR. JEFFRAY, seconded by DR. BRYCE, it was remitted to the Council to revise the Rules of the Society.

Several objects of scientific interest were exhibited by Mr. Mayer and other members.

November 30, 1864.—*The* PRESIDENT *in the Chair.*

Mr. William Moffat, junr., Merchant, was elected a member of the Society.

DR. BRYCE and the REV. HENRY W. CROSSKEY made a joint communication to the Society "On Certain Described Cases of the Occurrence of Fossils in the Boulder Clay of the West of Scotland."

December 14, 1864.—DR. ALLEN THOMSON, *Vice-President, in the Chair.*

The VICE-PRESIDENT, as Local Secretary in this city to the Ray Society, called attention to the new issue of books by that body, and recommended it to the support of the members of the Philosophical Society, who, under the new arrangements, would receive for their annual subscriptions a series of original scientific works of the highest value. These publications would in future appear with greater regularity than for some years past.

The following papers were read:—

"On the Mechanical Action of Glaciers in Excavating Lakes," by Mr. John D. Campbell.

"On Evidences of Glacial Action in the neighbourhood of Loch Doon," by Dr. Bryce.

January 11, 1865.—Rev. Mr. Crosskey *in the Chair.*

Mr. George C. Foster exhibited and described Mechanical Models illustrative of some of the Fundamental Laws of Optics.

Dr. Bryce stated the result of an excavation made at Kilmaurs for the discovery of the bed in which the remains of Elephants (believed to be *Elephas primigenius*) were found in the years 1817 and 1825. The fossils were discovered under the Boulder Clay, in a deposit which Dr. Bryce considered to be of the age of the English Crag.

Dr. Bryce called the attention of the Society to specimens of Metallic Ores from the neighbourhood of Loch Fyne.

January 25, 1865.—Dr. Allen Thomson, *Vice-President, in the Chair.*

Mr. Robert Townsend, Chemist, was elected a member of the Society.

The Vice-President congratulated the Society on the presence of Dr. J. P. Joule, of Manchester, one of the Society's honorary members.

Professor William Thomson explained the defects of the ordinary Electrometer in its want of assured discrimination between positive and negative electricity; and exhibited and described various new instruments of greater delicacy, to which he gave the name of Heterostatic Electrometers.

Dr. Joule expressed his gratification with the instruments exhibited, and stated that many most important points in their construction were entirely new.

February 8, 1865.—Dr. Francis H. Thomson, *Vice-President, in the Chair.*

Dr. Thomas Anderson read a paper "On some Points connected with the Economy and Use of Sewage."

The paper gave rise to a discussion, in which Dr. Gairdner, Mr. Downie, Mr. Stamford, and Mr. Macadam took part.

February 22, 1865.—DR. FRANCIS H. THOMSON, *Vice-President, in the Chair.*

The following were elected members of the Society, viz. :—

Mr. Alexander Macarthur, 10 Woodside Terrace.
Mr. John Martin, 5 Park Gardens.
Mr. William Gentles, 2 Windsor Place.

MR. GEORGE C. FOSTER delivered a lecture "On the Use of the Prism in Chemistry, including Spectrum Analysis." The lecture was illustrated by numerous experiments.

A cordial vote of thanks was given to Mr. Foster.

March 8, 1865.—DR. FRANCIS H. THOMSON, *Vice-President, in the Chair.*

Mr. Thomas Wiseman was elected a member of the Society.

The VICE-PRESIDENT intimated that the Council had devoted much of its attention during the winter to the revision of the Regulations of the Society, and would be prepared to lay its report before the next meeting.

PROFESSOR GRANT gave an exposition of the present state of our knowledge of the Sun's Distance from the Earth, with remarks on the different methods which have been proposed for its determination.

March 22, 1865.—DR. FRANCIS H. THOMSON, *Vice-President, in the Chair.*

Mr. Charles S. Boog was elected a member of the Society.

The following books were presented by Mrs. Hastie, of Carnock, through Mr. James R. Napier, to the Society's Library:—

1. Papers and Correspondence relating to the Arctic Expedition under Sir John Franklin, 1848–50.
2. Report on Public Libraries. 1850.
3. Papers relating to a Court-Martial in Ceylon.
4. New York Directory for 1812.
5. General Report on Manufactures, Navigation, Commerce, &c., of Scotland. By J. Boaz. 1813.

The thanks of the Society were voted to Mrs. Hastie for her donation.

The VICE-PRESIDENT presented a draft of the Regulations of the Society as revised by the Council, and bore his testimony to the valuable services rendered by Mr. James R. Napier as Convener of the Council's Committee on the Regulations, in which capacity he had during the winter bestowed much time and attention on the duty assigned to the Committee.

The Secretary having read the draft, of which copies were furnished to the members present,

The Vice-President directed attention to the principal alterations introduced, and, requesting the members to give the draft a careful perusal before the next meeting, proposed that the first vote be taken to-night. He therefore moved the approval of the report and adoption of the revised draft; which, being seconded by Mr. James R. Napier, and submitted to the Society, was unanimously agreed to.

Professor William Thomson made a communication "On Secular Variations of Terrestrial and Atmospheric Temperature."

April 5, 1865.—Dr. F. H. Thomson, *Vice-President, in the Chair.*

Mr. Robert Hogg, 9 Newton Street, was elected a member of the Society.

The amended Regulations of the Society were considered in detail, and, on the motion of the Vice-President, seconded by Mr. James R. Napier, were unanimously and finally adopted.

The following papers were read and illustrated by Dr. John Taylor, viz.:

1. "On a Means of Preventing Accidents in Diving Bells."

2. "On an Electric Apparatus for Detecting the Presence of Pieces of Metal, such as Musket Balls, &c., in Gunshot Wounds."

April 19, 1865.—Dr. F. H. Thomson, *Vice-President, in the Chair.*

The following papers were read:—

Mr. William Gorman,—"A Description of a Heat-restoring Gas Furnace, constructed for Heating Iron, according to the Patent of William Gorman and John Paton, with Results and Observations."

Rev. H. W. Crosskey,—"New Additions to the Fauna of the Glacial Epoch, by Rev. H. W. Crosskey and Mr. David Robertson." (This paper will be printed in the next Part of the *Proceedings.*)

The Vice-President announced that the session of the Society terminated with the present meeting. He expressed his regret at the continued absence of the President, from infirm health. In looking forward to the next session, he indulged the hope that the proceedings of the Society would acquire fresh interest from the introduction of public lectures, for which provision has been made in the revised Rules. He took leave to recommend to the members that, in following out their pursuits during the summer, they would bear in mind what was due to the Society by storing up information to be communicated when they re-assembled next winter.

MINUTES CONNECTED WITH THE EXHIBITION OF 1846-47.

Printed by order of the Council of the Philosophical Society.

Extracts from Minutes of PHILOSOPHICAL SOCIETY OF GLASGOW.

23d March, 1846.—Mr. Liddell moved, in accordance with a recommendation from the Council, the appointment of a Committee from this Society, to co-operate with any Committee that might be named by the Town Council, in arranging for a Public Exhibition of Models of Machinery, Specimens of Geology, Mineralogy, &c.; and that a sum not exceeding £50 should be placed at their disposal as a guarantee against loss; which was seconded by Mr. Crum, and unanimously agreed to. The following gentlemen were then named: Messrs. Crum, W. Murray, Hastie, Gourlie, Keddie, Dr. R. D. Thomson, Messrs. Liddell and Bankier, with power to add to their number—Mr. Liddell, Convener.

1st April, 1846.—Mr. Liddell read the following Minute from the Joint-Committee of the Society and Town Council on the proposed Exhibition:—

" Glasgow, the twenty-seventh day of March, 1846. Convened " Andrew Liddell, Esq. (Chairman), Bailies Whitehead, Mackinlay, " &c., Messrs. Murray, Gourlie, &c., Members of the Committee as to " Exhibition of Models, &c.

" Having resumed consideration of the draft agreement submitted " at last meeting as to the terms on which the Exhibition should be " got up and conducted, and the same having been fully considered " and discussed, the following was unanimously approved of, and " agreed to be recommended to the Town Council and the Philo- " sophical Society for adoption, viz.:—

" *First,* That the Philosophical Society take the charge exclusively " of collecting and arranging all articles to be exhibited, and that it " shall be called " The Philosophical Society's Exhibition of Models, " &c."

" *Second,* That for the purpose of raising sums of money towards " defraying outlay, admission fee be exacted on the days and at the " rates now to be named (admission to be given to ladies on all the " days), viz.,—the Exhibition to be opened on the evening of Tuesday, " the 29th December, at Seven o'clock, being promenade night; Single " Tickets, 2s. 6d., Family Tickets of five, 10s., which includes tea,

"coffee, and music. Wednesday, the 30th, and Thursday, the 31st, "open from Eleven in the morning till Nine at night—admission, 1s. "each; children under ten years, 6d. each. Friday, 1st, Saturday, "2d, and Monday, 4th January, open to the public gratis, from Ten "morning till Nine at night.

"*Third*, That for the purpose of increasing the amount of funds "made available for paying expenses, the members of the Philoso- "phical Society waive all claim or right which they have for them- "selves or friends of being admitted on terms more favourable than "the public generally.

"*Fourth*, That the Philosophical Society guarantee against loss in "equal proportions with the Town Council, to the extent of £100; "and that if the loss should exceed the said sum of £100, the excess "to be borne exclusively by the Town Council, it being understood "that the gross expenditure shall not exceed £500.

"*Fifth*, If it should happen that, in place of a loss, there should "be overplus of money received, said overplus to be laid aside as a "fund for future exhibitions of a similar nature, it being understood "that in estimating the loss or gain, no rental is to be exacted for the "use of the City Hall, in which the Exhibition is to be made.

"*Sixth*, While the Philosophical Society takes exclusive manage- "ment of collecting and arranging all articles to be exhibited, yet it "must be understood that the Society goes into no single measure "involving an outlay of £50 or upwards, without approval of the "Committee of the Town Council.

"*Seventh*, That Corresponding Committees be appointed as soon "as possible in London, Edinburgh, Manchester, and Liverpool, for "the purpose of aiding in the collection of articles of interest fitted for "the exhibition; and that the Town Council as a Corporation, and "its members as individuals, agree to lend their aid also in this work.

"*Eighth*, That Glasgow College, Anderson's University, and "Glasgow Mechanics' Institution, be respectfully requested to give "from their museums such articles as may be considered proper and "fit to be removed for same purpose.

"Which report was unanimously agreed to."

15th April, 1846.—The Council reported that Mr. Liddell had intimated to the meeting this evening that the Town Council had agreed to the Report of the Joint Committee of the Town Council and the Society as to the Exhibition of Models, &c., giving to the Joint Committee discretionary powers as to details. Mr. Liddell then moved that this Society give the Committee the same powers, which was agreed to.

28th April, 1847.—Mr. Liddell made his concluding Report on the winding up of the affairs of the Society's Exhibition at the beginning of the year. All the accounts were paid, leaving a balance of receipts over expenditure (now in the Union Bank) of £453 8s. 10d., which, with £7 2s. 10d. of interest to the 20th of April current, leaves an available balance of £460 11s. 8d., to be laid aside for future Exhibitions of a similar kind, in conformity with Article V. of Contract Agreement betwixt the Town Council and the Philosophical Society, of date 1st April, 1846, which runs thus:—"If it should "happen that in place of a loss, there should be an overplus of money "received, said overplus to be laid aside as a fund for future exhi- "bitions of a similar nature."

Mr. Liddell moved, agreeably to a recommendation in the Acting Committee's Report to the General Committee on the Exhibition, and adopted by the latter on the 20th April, "That this money in "the meantime be lodged with the Corporation of the city, at the "current rate of interest, in name of the Lord Provost and Senior "Bailie of Glasgow, *ex officio*, and of the President and Vice-Presi- "dent of the Philosophical Society, also *ex officio*, as Trustees for "the application of this sum; and that the Treasurer of the Philo- "sophical Society for the time being be the custodier of the bill or "other voucher for the debt; and that he be requested to see that "the interest be added to the principal sum twice every year, at the "usual terms of Martinmas and Whitsunday, commencing at the "term of Martinmas, 1847. And, further, that the Treasurer be "required to report to the Philosophical Society, at least once every "year, the state of the Fund; and that the Philosophical Society see "that this report to them is regularly given in."

Which motion was unanimously approved of by the Society.

16th November, 1864.—The Treasurer reported that the money lent to the Corporation of Glasgow, from proceeds of Exhibition in 1846, amounted, with interest to 15th May last, to £937 7s. 2d.

EXTRACT *from Minutes of a Meeting of the* MAGISTRATES AND COUNCIL OF GLASGOW, *held on 26th May, 1847, the* HON. ALEX. HASTIE, *Lord Provost, in the Chair.*

BAILIE LIDDELL, with reference to the Report hereinafter engrossed, distributed among the Members of Council, noticed at some length the importance of raising an adequate fund for the establishment of a permanent Philosophical Exhibition in Glasgow, and concluded by moving: "That the Council take in loan the money

"now offered by the Committee of the Philosophical Society Exhi-"bition, amounting to £462 6s. 2d.; and that the Chamberlain be "authorized to grant a Bill in the usual terms for the same, in favour "of the Lord Provost and Senior Bailie, *ex officio*, and the President "and Vice-President of the Philosophical Society, also *ex officio*, as "Trustees for the purpose named in the Report." The Lord Provost noticed the humane and successful exertions made by Bailie Liddell and other Members of the Philosophical Institution on the occasion of the late Exhibition, and expressed the thanks of the Council to Bailie Liddell and the other gentlemen. Thereupon the Magistrates and Council unanimously approved of Bailie Liddell's motion, and authorized the Chamberlain to grant the usual promissory note for the sum, and upon the terms therein mentioned.

"REPORT.—The Acting Committee to the General Committee of "the Philosophical Society's Exhibition.

"The Acting Committee in this their final Report do not consider "it necessary to go into a lengthened detail of the proceedings connected "with the Exhibition, this having been already done in a Statistical "Account drawn up at their request by their Chairman, and a printed "copy of which has been sent to every member of the General "Committee. They consider that they have now only to report that "all matters connected with the Exhibition are brought to a close. "Every article sent for the purpose of being exhibited has been "received back by its owner. All the accounts are paid, and there is "a balance of receipts over expenditure, now in the Union Bank, "amounting to £453 8s. 10d., which, with £7 2s. 10d. of interest "to the 20th April current, leaves an available balance of £460 11s. 8d. "to be laid aside for future exhibitions of a similar kind, as per "Article V. of Contract Agreement betwixt the Town Council and "the Philosophical Society, of date 1st April, 1846, which runs thus:— "'If it should happen that in place of a loss there should be an over-"'plus of money received, said overplus to be laid aside as a fund for "'future exhibitions of a similar nature.' Your Committee take the "liberty of recommending that this money in the meantime be lodged "with the Corporation of the city at the current rate of interest, "which is Five per Cent., in name of the Lord Provost and Senior "Bailie of Glasgow, *ex officio*, and of the President and Vice-President "of the Philosophical Society, also *ex officio*, as Trustees for the appli-"cation of this sum in conformity with the above-quoted Article, and "that the Treasurer of the Philosophical Society for the time being "be the custodier of the bill or other voucher of the debt, and that

"he be requested to see that the interest be added to the principal "sum twice every year, at the usual terms of Martinmas and "Whitsunday, commencing at the term of Martinmas next ensuing, "1847; and, further, that it be a recommendation of this Committee "that the Treasurer be required to report to the Philosophical Society "at least once every year the state of this Fund, and that the "Philosophical Society see that the Report to them is regularly given.

"Your Committee cannot close this Report without expressing their "high satisfaction at the eminent success which attended this Exhibi- "tion, and their conviction that the patronage of the Town Council, "with its liberality in guaranteeing the General Committee against "the greater part of any loss that might have occurred, as also their "granting the City Hall for the Exhibition rent free, gave an impetus "to the skilful carrying out of the plan by the willing and industrious "working members of the Philosophical Society. To the active "exertions of the latter for a long period of time, and to their thorough "knowledge of the proper objects for exhibition, and their interesting "their scientific friends in other places, from whom many valuable "contributions were received, we are mainly indebted for that which "gave so much and such general satisfaction. There were many "other Contributors unconnected in any way with either of the above- "named bodies to whom the Committee were indebted for many "valuable and rare articles. To all the Contributors a special vote of "thanks of the Committee has been sent. In conclusion, allow your "Acting Committee to express their hope that a permanent Exhibition "of a similar kind will be at no distant period established in this city.

"It will be seen from the Article quoted above that something of "this kind was in view of the promoters of this Exhibition now "passed away, when taking preliminary steps for its being got up.

"The contingency of a surplus of revenue over expenditure which "they provided for has actually taken place to the amount of nearly "Five Hundred Pounds. It will be seen that this sum can be "devoted to no other purpose, and it may prove a valuable nucleus "from whence may arise a similar concern, but of a more durable "description, and one which may prove to be not only attractive, but "to the highest degree useful to the population of this great commer- "cial city.

"(Sig^{d}) ANDREW LIDDELL, *Chairman.*

"*April 20, 1847.*"

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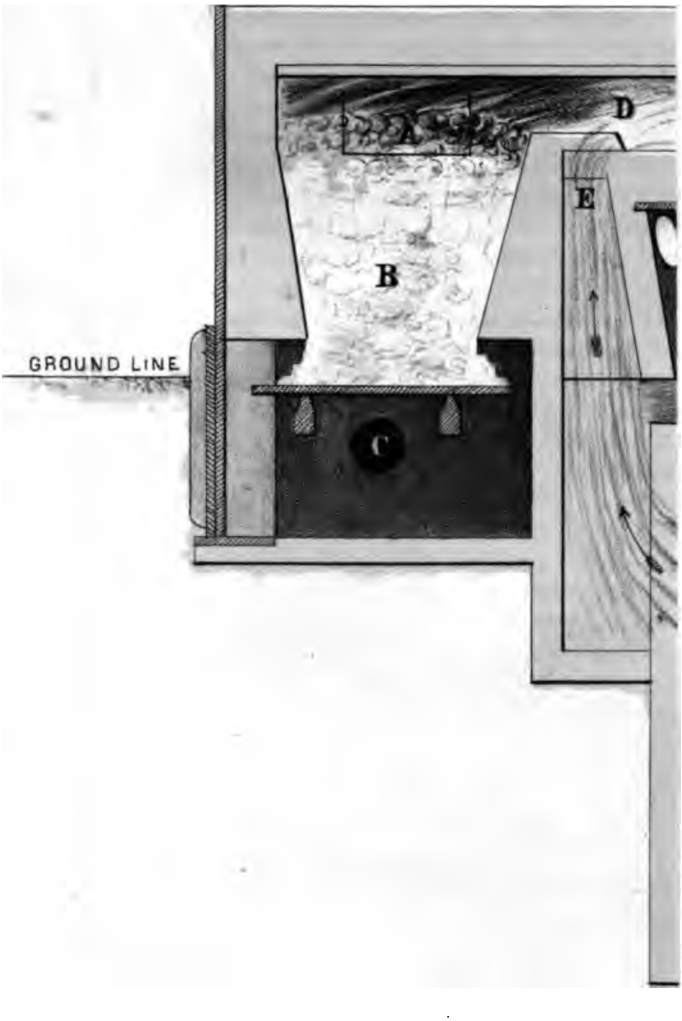


PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

SIXTY-FOURTH SESSION.

I.—*On the most Profitable Speed for a Fully Laden Cargo Steamer for a given Voyage.* By Mr. James R. Napier.

Read December 13, 1865.

The original title given to this paper, "The most Profitable Speed for Cargo Steamers," was too extensive, and the present title is perhaps still too large for all that I have attempted, which will be better understood from the following statement:—

Having been asked to send that now notable steamer of mine, the "Lancefield," from Glasgow to Dunkirk with cargoes of pig-iron, to bring back sugar, I had to satisfy myself that the voyages would be profitable; and if entered upon, I desired that the profit should be the greatest that was possible. As I was aware that it would vary in some way with the velocity of the vessel, I set to work to find a general expression for the law of its variation, and also the velocity, if any, which would yield the maximum of profit. The communication this evening is the result of the inquiry.

I have little excuse, however, for bringing it forward, except that it is perhaps new; for the solution of the problem, at least in its limited state, is too simple to escape any mathematical student whose attention is drawn to it.

As the problem refers to *a* steamer for a given voyage, the following particulars of the vessel, cargo, and voyage are necessary, and supposed to be known. I have chosen also the case where the steamer takes coals from the port of departure sufficient to bring her back.

L, the gross load of coals, cargo, and stores, which the vessel can carry;

D, the length of the voyage for which she has taken coals;

A, a velocity of the loaded ship produced by the burning of a known rate of coals;

h, the rate at which the coals are burned to produce the velocity A;
a, the rate of cost of the coals, on board;
b, the rate of cost of loading and unloading a given cargo;
c, the rate of cost of the dues on a given cargo;
e, the rate of commission on the freight carried;
F, the rate of freight for the outward cargo;
G, the rate of freight for the inward cargo;
H, the rate of hire, for the vessel and crew's wages;
J, the time lying in port loading, unloading, &c.;
K, all other expenses of the voyage, such as dues on the vessel, pilotage, crew's wages, and hire of vessel while in port, &c.;
Let V be the velocity sought;
and X the weight of coals for the voyage, whose length is D;
Then, as X varies as V^2, and as h tons are burned at a velocity A,

$$X = \frac{h\, V^2\, D}{A^3};$$

$$\frac{D}{V} = \text{the time occupied in steaming the distance D;}$$

$$L - \frac{h\, V^2\, D}{A^3} = \text{weight of the outward cargo;}$$

$$L - \frac{h\, V^2\, D}{2\, A^3} = \text{weight of the inward cargo;}$$

$$F\left(L - \frac{h\, V^2\, D}{A^3}\right) = \text{amount of the outward freight;}$$

$$G\left(L - \frac{h\, V^2\, D}{2\, A^3}\right) = \text{amount of the inward freight;}$$

$$L\,(F + G) - (2\,F + G)\,\frac{h\,D}{2\,A^3}\,V^2 = \text{amount of the freight for the whole voyage out and in;}$$

$$\frac{a\,h\,D}{A^3}\,V^2 = \text{the cost of the coals for the voyage;}$$

$$b\left(L - \frac{h\,D\,V^2}{A^3} + L - \frac{h\,D\,V^2}{2\,A^3}\right) = 2\,b\,L - \frac{3\,b\,h\,D}{2\,A^3}\,V^2 = \text{cost of loading and unloading two cargoes;}$$

$$c\left(L - \frac{h\,D}{A^3}\,V^2\right) = \text{cost of the dues on one cargo;}$$

$$e\,F\left(L - \frac{h\,D}{A^3}\,V^2\right) + e\,G\left(L - \frac{h\,D}{2\,A^3}\,V^2\right)$$

$$= e\,L\,(F + G) \times (2\,F + G)\,\frac{h\,D}{2\,A^3}\,V^2$$

$$= \text{cost of commissions;}$$

$\frac{H\,D}{V}$ = cost of the hire for vessel, and wages while steaming the distance D.

These are all the items of profit or loss in reference to this particular voyage that I know of that would be affected by the speed of the vessel; if there are others, they can be added. In reference to the quantity H, or the rate of hire for the vessel, &c., it is made up of sums for insurance, depreciation, and repairs of the vessel. Although all these items vary with the velocity of the vessel, according to laws of their own, and ought to enter such an investigation as I am making, I have not at present gone so delicately to work; and in regard to the quantity, X, of coals for the voyage, which I have assumed = to $\frac{h\,D\,V^2}{A^3}$, it is derived from the practically true statement that the *power* for a given velocity varies as the cube of that velocity. As, however, the power itself may be got in various ways, and in some more economically than in others, *e. g.*, varying the ratio of expansion in cylinders properly constructed; but here, again, I have not gone so delicately to work as perhaps I might have done to have made the investigation sufficiently general, because, in my own vessel, the primary object of this inquiry, the ratio of expansion is at its greatest limit when the vessel is going fastest, and I can do nothing more for a slower velocity than shut the throttle valve, or keep it open and reduce the boiler pressure; and the ordinary cargo steamers are in general so constructed that, although they may have the means of working with a greater ratio of expansion, they gain nothing by it, and perhaps lose. With this understanding as to the means for reducing, if necessary, the speed, the value of X given above will be sufficiently near the truth for a practical purpose.

The profit on the voyage, being the difference between the amount received for freight and the expenses incurred in earning it, will therefore be

$$L\,(F + G) - (2\,F + G)\frac{h\,D}{2\,A^3}\,V^2 - \frac{a\,h\,D\,V^2}{A^3} - 2\,b\,L + \frac{3\,b\,h}{2\,A^3}\,D\,V^2$$

$$- c\,L + \frac{c\,h\,D}{A^3}\,V^2 - e\,L\,(F + G) + e\,(2\,F + G)\frac{h\,D}{2\,A^3}V^2 - \frac{H\,D}{V} - K;$$

$$= L\left\{(F + G)\,(1 - e) - (2\,b + c)\right\} - \left\{(2\,F + G) + 2\,a - 3\,b - 2\,c\right.$$

$$\left. - e\,(2\,F + G)\right\}\frac{h}{2\,A^3}\,D\,V^2 - \frac{D\,H}{V} - K \ldots\ldots\ldots (A);$$

and this, divided by the time occupied in making the voyage— viz.,

$\frac{D}{V} + J$,—will be the profit in a unit of time, or

$$Q = \frac{L\,R\,V - S\,D\,V^3 - D\,H - K\,V}{D + J\,V};$$

$$\text{or, } Q = \frac{(L\,R - K)\,V - S\,D\,V^3 - D\,H}{D + J\,V}$$

which is to be a maximum,

R being taken for the co-efficient of L in (A),
S being taken for the co-efficient of D V^2 in (A),
Q being taken for the profit:
Therefore

$$\frac{(D + J V)\{L R - K) - 3 S D V^2\} - \{(L R - K) V - S D V^3 - D H\} J}{(D + J V)^2} = 0;$$

$$D (L\,R + J\,H - K) - 3\,D^2\,S\,V^2 - 2\,J\,S\,D\,V^3 = 0;$$

$$V^3 + \frac{3\,D\,V^2}{2\,J} - \frac{(L\,R + J\,H - K)}{2\,J\,S} = 0;$$

a cubic equation which can be easily solved by any of the ordinary processes when the numerical data are supplied.

Read November 29, 1865.

Since last meeting I have thought it desirable to substitute H J + M for K, H J being the amount of the crew's wages and hire of the vessel while in port, and M the amount of all the other charges—charges which are not dependent on the quantity of cargo—such as harbour dues on the tonnage of the vessel, and pilotage, stamps, forms, protests, &c., &c.

The profit will therefore be

$$Q = \frac{(L\,R - J\,H - M)\,V - S\,D\,V^3 - D\,H}{D + J\,V} \quad \text{.......(B);}$$

and the cubic equation giving the most profitable velocity

$$V^3 + \frac{3\,D}{2\,J}\,V^2 - \frac{L\,R - M}{2\,J\,S} = 0. \quad \text{...............(C).}$$

To illustrate the application of these formulæ, the following data may be assumed for the "Lancefield" for the Dunkirk voyage:—

L = 240 tons;
D = 1,428 nautical miles:

$A = 9\tfrac{1}{2}$ nautical miles an hour;
$h = 0{\cdot}4$ tons;
$a = £0{\cdot}5$ per ton;
$b = £0{\cdot}05$;
$c = £0{\cdot}05$ per ton;
$e = £0{\cdot}07$ per pound;
$F = £0{\cdot}55$ per ton;
$G = £0{\cdot}7$ per ton;
$H = £0{\cdot}31$ per hour;
$J = 168$ hours (7 days);
$M = £38$;
Then, $R = 1{\cdot}0125$;
$S = 0{\cdot}000554$; and

$$V^3 + \frac{3\,D}{2\,J}V^2 - \frac{L\,R - M}{2\,J\,S} = V^3 + 12{\cdot}75\,V^2 - 1{,}101 = 0;$$

and V the most profitable velocity $= 7{\cdot}4$ nautical miles an hour.

The following table shows the effect that the velocity of the ship has on the profits, &c., &c.:—

V, Velocity of ship nautical miles per hour,	9·4	7·4	5·4
Q, Profit per hour $= \frac{153\,V - {\cdot}791\,V^3 - 443}{1{,}428 + 168\,V}$ £	0·112+	0·138+	0·111−
X, Coals for the voyage $= \frac{2}{3}V^2$, tons	58·9	36·5	19·44
Weight of outward cargo $= 240 - \frac{2}{3}V^2$ tons	181·1	203·5	220·56
Profit in 300 days, £	806·4	993·6	799·2
The relative powers required to produce the velocity ·P ∝ V^3,......................	2·05	1·	0·39

As the "Lancefield" can steam at the rate of about $9\tfrac{1}{2}$ nautical miles an hour, the foregoing results show that a vessel for this trade, of about half her power, carrying the same load, would be more profitable.

At the same time, however, if J, or the time the vessel is in port loading and unloading, &c., can be diminished without increasing the cost of the loading and unloading, the most profitable speed will increase as in the following examples, one of which is the impossible one of the vessel being loaded and unloaded in no time:—

J, or time in port,...................... hours	168	144	120	96	0
The most profitable speed,......................	7·4	7·6 —	7·8 —	8· —	9·28

It is also apparent from (C) that the more the value of M decreases the more the most profitable velocity increases. To take the improbable case of M being equal to nothing, or there being no tonnage dues on the vessel, or pilotage, &c., &c., and for J, or the detention in port, 96 hours, the most profitable velocity would be 8·6 nautical miles an hour.

These calculations show that in order to make the most profit out of a given amount of capital in steam-ships, they should be built with special reference to the trade in which they are to be employed. And as the resistance of water to floating bodies is sufficiently well known for the purpose, a skilful architect can, with little trouble, design a vessel to carry a given load for a trade of which all the data are given as above, which vessel would be more profitable than any other of similar construction, but of a different velocity, consequently of a different size and power: this is what ought to be done; and this, I think, is what shipowners ought to desire.

II.—*The Luminosity of the Sea.* By Messrs. David Robertson and William Keddie.

Read November 29, 1865.

The phenomenon of luminosity, or phosphorescence (as it has been termed, from a vague idea of its being caused by a slow combustion of phosphorus), is exhibited by various bodies, inorganic and organic. Numerous minerals possess the property of emitting light, a circumstance which appears to have been first noticed by the celebrated Benvenuto Cellini, the sculptor, early in the sixteenth century, and subsequently by Boyle, Wedgwood, Haüy, and Brewster, by whom the observation has been extended to a great variety of mineral substances. Some of the softer woods are known to become luminous when in a state of decay; and a similar appearance is occasionally seen in peat-mosses. Many insects, including the familiar instances of the glow-worm and the fire-fly, exhibit a high degree of luminosity. Light was developed in the body of a molluscan animal, by M. Milne Edwards, in an unexpected manner. On putting some living specimens of pholas in alcohol, he saw luminous matter oozing out of the bodies of the molluscs, which descended through the fluid by its own

weight, and extended itself in a stratum of vivid light at the bottom of the vessel. The phenomenon, as it occurs in the mineral, the vegetable, the insect, and the mollusc, is probably due to a different cause; and it is far from being certain that the same cause is common to luminous insects and luminous marine animals, to which latter the phosphorescence of the sea is ascribed, and to which the present inquiry will be restricted.

The cause of the phenomenon is involved in much obscurity. The luminosity of the sea has been a subject of interest from early times. It is incidentally alluded to by Pliny, who was disposed to ascribe it to the action of some kinds of fish. In the writings of Thomas Hobbes, which appeared in the middle of the seventeenth century, that quaint old philosopher gives the following explanation of "the cause of light in the concussion of the sea-water":—"Also," says he, "the sea-water shineth when it is either dashed with the strokes of oars or when a ship in its course breaks strongly through it; but more or less according as the wind blows from different points. The cause whereof may be this, that the particles of salt, though they never shine in the salt-pit, where they are but slowly drawn up by the sun, being here beaten up into the air in greater quantities and with more force, are withal made to turn round, and consequently to shine, though weakly. I have, therefore, given a possible cause of this phenomenon." At the beginning of the present century, Dr. Hulme published his views on the subject in the *Philosophical Transactions*, referring the light to the diffusion in the water of a mucus secreted by certain fishes. For a long period the notion prevailed in the popular mind, and was even shared in by some naturalists, that luminosity was a property inherent in sea-water. Such is, indeed, still the opinion of many sailors and fishermen, who have long been in the habit of prognosticating storms from brilliant displays of phosphorescence. Hitherto the subject had engaged little of the attention of naturalists, and had been discussed only in a slight and incidental manner. But about half-a-century ago, the late Dr. John Macculloch, the geologist, whilst prosecuting his survey of the Western Islands of Scotland, began to devote his leisure to the investigation of the sources of luminosity in the sea. He made many careful observations and experiments, the results of which were given in the second volume of his *Description of the Western Islands*, published in 1819, and afterwards expanded into the elaborate article on "Phosphoresence," which appeared in Brewster's *Encyclopœdia* in 1830. In the latter form the article was probably exhaustive of the state of the inquiry as it stood thirty-five years since. Later notices have usually been of a casual and incidental

description, as before, and have added comparatively little to the general results of Macculloch's inquiries. Before mentioning some observations made by the authors of this paper last autumn, and for the purpose of briefly showing the former and present progress of the investigation into this curious subject, the Society will perhaps extend its indulgence, first, to a *résumé* of Macculloch's results, and, secondly, to a few notices of later observations and researches.

Macculloch showed, in the first place, that the phosphorescence of sea-water is a property not belonging to itself, but to substances accidentally contained in it. Amongst these substances he attached little importance to the presence of the decomposing matter of fishes, to which many were disposed to ascribe the luminosity of the water. At a certain period after the death of fishes they become luminous, before putrefaction commences; nay, this property actually disappears before the putrefactive process has set in. During this luminous stage, as well as after it, there is a solution of part of the solid matter of the fish, or at least a disintegration of the cellular or muscular fibre. For some time it retains the phosphorescent quality, provided it was in a phosphorescent condition previously; and in this way a portion of the water may be rendered luminous for a short time, merely by agitating the luminous fish in it,—a circumstance explaining the theory of Hulme, which had led to the belief that the luminous appearance of sea-water was occasioned by the presence of putrescent matter. In this case, however, there is no putrescence, in the ordinary sense; besides, the light is very transitory, and rapidly disappears. Dr. Macculloch often succeeded in obtaining water, thus turbid and abounding in fibrous matter, which was utterly dark or incapable of phosphorescence, unless the water contained living animals, and then it was invariably luminous. There is no reason to doubt that in certain circumstances the water of the sea may be rendered luminous by dead phosphorescent matter diffused through it, whether proceeding from dead fishes, or occasionally from the bodies of living ones. But the quantity of such matter must always be limited, and cannot be diffused far from its source; and although a certain degree of luminosity may be due to this cause, it is inadequate to account for the universality of the phenomenon, when the entire surface of the water is seen flashing into light. Distinguishing the light into three kinds, —namely, first, a pale and feeble whitish light; secondly, wide flashes resembling a particular kind of lightning; and thirdly, brilliant sparks of different sizes and colours,—the author affirms that it is only the first of these, or the faint diffused light, which is caused by dead matter, although not exclusively, since numerous observations prove

that a similar effect is produced by the presence of minute animals. The writer's conclusion on this part of the inquiry is, in effect, that the degree of phosphorescence produced by dead animal matter is so inconsiderable that it need scarcely enter as an element into the estimation of the causes of the luminous appearance.

It had been confidently affirmed by many observers that they had witnessed both the general diffused light and the sparkling appearance in sea-water where no animals were present. The author proves this assertion to be a result of erroneous observation, or a mistake of negligence or ignorance. His own experiments established the fact of the phosphorescence being invariably caused by the presence of animals, mostly so minute as to be discernible only by the microscope, and many of them so transparent as readily to escape detection by an inexpert observer. Amongst the sources of error pointed out by the writer, he mentions that luminous animals themselves contrive to deceive the observer. There is no doubt, he remarks, that the light is under the command of the animals, and is from this and other causes liable to intermissions; and he adds a fact well known to every observer, that whether from caprice, or fatigue, or a voluntary effort, or from some other cause yet unknown, they often refuse to show their light even when violently agitated. In the course of our nocturnal operations with the surface and dip-net, we have ourselves taken millions of microscopical entomostraca, and other animals invisible to the naked eye, the entrance of which into the net was accompanied by a constant blaze of the most brilliant phosphorescence; and although the aggregate was so enormous as to become as palpable as a quantity of sand, it was rarely found that more than a few faint scintillations were produced by agitating the water in which they were preserved for inspection.

When Dr. Macculloch made his observations, naturalists had scarcely begun to examine the smaller marine animals, and out of 200 species detected by him in six summer weeks on the western shores, two-thirds were undescribed species. Much has been done since his time to remove this opprobrium from marine zoology, which, with the facilities afforded by improvements in the microscope, has made rapid advances within the last few years; and the progress of investigation tends to confirm the general conclusion at which Macculloch arrived, that the property of phosphorescence, so far from being, as was at one time supposed, confined to a few worms and medusæ, extends, in all probability, to all the inhabitants of the ocean. The power of emitting light would appear, however, not to be possessed by marine animals in proportion to their magnitude; for it is established by observation

that the greatest brilliancy is produced by the united action of creatures of microscopical minuteness dispersed throughout the ocean in inconceivable profusion. Macculloch imagined that the end to which the phenomenon is subservient in the economy of marine life was "mutual communication." "As far as the wants of these animals are concerned," he remarks, "this light is a substitute for that of the sun; and as their great and hourly wants are mutual self-preservation, or alternately prey or defence, so by these lights they are guided to each other for attack, while by their power of obscuring them, they are furnished with the means of resisting it." This ingenious fancy overlooks the circumstance of the phosphorescence varying with the seasons, being chiefly remarkable in autumn. It is even then by no means constant, but, on the contrary, fluctuating and temporary; and in some seasons, as in the dead of winter, is seldom or never perceptible on our shores. So long as we remain in ignorance of the efficient cause of the phenomenon, it seems at least premature to speculate on a final cause.

The phenomenon of luminosity is peculiar to no sea, being observed in the frozen ocean of either pole, in the tropics, and in the Atlantic and Pacific. Many of the animals by which it is produced must therefore be independent of climate. Mr. Gosse mentions that the most brilliant display of luminosity he ever witnessed was in the Gulf of Mexico, where the mass of water rolled from the ship's bows as white as milk, studded with innumerable sparkles of blue light, which he ascribes to different species of living animals, giving rise to variations in its aspect. In a voyage to India, Dr. Baird observed two kinds of light,—one produced fitfully by the darting of a cyclops through the water, and bright luminous globes caused by medusæ. The scintillations of light he traced to minute entomostraca. On the other hand, Professor Ehrenberg, of Berlin, mentions many animals which he found to be luminous in the Red Sea: but he could not detect the slightest light from any species of the entomostraca. Small medusæ were found to contribute to the general effect. It will appear from the subsequent statement that, for many weeks together during last autumn, the luminosity on the shores of Cumbrae was caused almost exclusively by species of entomostraca. The late Edward Forbes referred the phosphorescence chiefly to the presence of medusæ. These are generally autumnal animals, occurring occasionally in vast numbers on the coast, and exhibiting in a marked manner the luminous property. But during last autumn, with the exception of one of the larger species (*Cyanæa Lamarckii*), which was often stranded on the Cumbrae shores in blowy weather, the number of medusæ, of all

sizes, was notably small, and the phosphorescence must therefore have been only in a very inferior degree occasioned by the presence of these animals. Amongst incalculable swarms of entomostraca, a patient examination with the microscope led to the detection of only a very few of the smaller medusæ.

A note on phosphorescence in some annelides and ophiuræ (brittle stars), from the pen of M. de Quatrefages, appeared in the *Annales des Sciences Naturelles*, in 1843, containing a variety of careful observations made on the shores of the British channel. In the open sea he found the light to be limited to occasional but vivid scintillations, occasioned by the stroke of an oar or the bow of a boat, when the sparkles were extinguished as suddenly as they had been produced. But in some circumstances he found the fucus bordering the shore to present the appearance of a sheet of incandescence. In order to bring this singular spectacle into play, it was sufficient to agitate strongly some of the fronds which had been for a short time left uncovered by the sea, when the entire surface seemed to glow under his hand. The animals which emitted this light were observed to be chiefly annelides, but small ophiuræ were equally brilliant. He also satisfied himself that some microscopical entomostraca possess the faculty of becoming momentarily luminous; and to these, above all, was he disposed to attribute the bright sparkling appearance which he had witnessed on several parts of the coast: at least, in examining, with the greatest care, a quantity of water taken from one of these localities, and which continued to give scintillations in the vessel, he never detected any other animal but microscopical entomostraca. The observations described by the author, however, relate, in the first place, to small species of Nereis, and also to worms of the genera Syllis and Polynöe. One of the annelides was found in the test of a balanus, and was about twelve lines long, and scarcely a fourth part of a line in diameter. Its feet were numerous, and arranged closely together. When he touched it accidentally, it began to move with rapidity, and at the same time became luminous to such a degree as to be clearly perceptible in the bright shining of a lamp, the light emitted from the body of the annelide being of a strongly-marked greenish tint. When placed in the dark, the animal appeared to be luminous in all its length; but on being examined with a power of five or six diameters, the light was seen to break up into points forming two parallel lines along the length of the body, and corresponding to the feet of the annelide. These luminous points were very brilliant, and seemed to move; they disappeared when the animal was in a state of repose, and shone anew when the animal was excited into activity. The author was led to the conclusion, after numerous

experiments, that among certain annelides the motor muscles of the feet are the exclusive seat of the phosphorescence; the appearance of the light coincides invariably with the contraction of the muscle, and invariably disappears with it. Among these animals, he adds, light is produced by scintillations, independently of any material secretion. This phenomenon resembles nothing which is observed amongst insects, where the luminous organ, distinctly marked, seems almost entirely formed of a network of small tracheæ; where the light has a remarkable fixity; where the phosphorescent matter may be collected after the death of the animal, and preserves its properties after it has been isolated. The phosphorescence of the annelides bears no resemblance whatever to that of the pholades, the medusæ, &c., since, in the case of the latter animals, it depends on the presence of mucus, which may be collected in considerable quantities. The same remarks apply to the ophiuræ. The arms of these creatures are composed of little calcareous pieces articulated the one to the end of the other, like the vertebræ in the tail of a lizard. The living matter which they cover is not merely a homogeneous substance, an animal pulp, as some authors have described it; for diverse tegumentary layers are distinguished in it; and the solid pieces are jointed by true muscular bands, the fibres of which are perceptible under the microscope. It is upon these points alone that the phosphorescence is manifested; it appears upon them in scintillations; the striæ which it seems to form are in the same direction as the fibres; it only appears when the arm is in motion; when the animal is at rest, no trace of luminosity is distinguishable, even when the parts are irritated. It thus appears that in the case of the ophiuræ, as in the annelide, the light is produced upon the muscular parts during contractions only, beyond the contact of the air, and independent of all secretion properly so called. Repeated action soon diminished the intensity of the light, which ultimately was extinguished, showing that its production fatigued the animal, and speedily induced exhaustion. Quatrefages is inclined to the belief, but apparently without adequate facts to support him, that the flashes of light in these instances are of the nature of electrical discharges. The methods adopted by this observer, however, indicate the kind of experimental researches fitted to remove much of the obscurity still resting upon a subject which he regrets has been too much neglected by naturalists.

In prosecuting the study of marine invertebrate animals, it is necessary to resort to many different appliances. Of these the dredge is the most efficient; but some of its auxiliaries, although they may appear trifling in themselves, may still have important bearings on the

significance of the results. The late Professor Edward Forbes, when dredging in Lochfyne, met with many dead shells of *Pecten Danicus*, and scarcely any living specimens. Hence he inferred that the animal was lingering on its northern limits, and exhibiting the rarity which precedes extinction in the waters of our shores. The same inference has been lately employed by an able writer in support of some theoretical views on the glacial epoch. Now, it has been found by Mr. Robertson that, by using another appliance than the ordinary dredge, hundreds of living specimens could be taken at a time in the same loch. This was accomplished by the simple expedient of extending a net over the bottom inhabited by them, and which they clasp by the meshes, and are thus readily secured. Sometimes valuable specimens of different kinds of animals are brought up entangled or hooked on the fisherman's long line, from rocky or stony ground, where no net or dredge could possibly be wrought. Other classes of animals live under the shelter of stones extending far beyond tide-mark, and out of the reach of any method of capture which has yet been devised; yet many even of these may be taken by means of another auxiliary to the dredge —namely, the stomachs of fishes, to which naturalists are often indebted for some of their rarest specimens. Apart from the methods of exploring the depths of the sea, the upper currents are examined by means of the surface-net, carefully constructed so as to retain the smallest animals floating in the water. This instrument was assiduously employed by us last autumn, and, perhaps for the first time, in making systematic observations after sunset, when the sea was in a state of phosphorescence. By comparing the produce of the net during the day and at night, we found that after sunset it was doubled or quadrupled both in number and variety. This was the constant result, day after day, and night after night, of working over the same course, embracing a considerable extent of the shores of Cumbrae. From the many trials we made, we are warranted in estimating the presence of microscopical and other small animals near the surface of the water to be at its minimum in the morning, at its medium or average in the afternoon, and at its maximum after sunset. There was a remarkable recurrence of the same species at the same periods of observation on successive days and nights, although this was not without exceptions. The gathering of one night generally contained all the species of the previous night; but on repeated occasions different species presented themselves in great numbers, more especially larval forms. On one evening in the month of August the gathering contained myriads of young crabs, very minute in size, and exhibiting one of the curious stages of their metamorphosis. Subsequently these became so rare that scarcely half-a-

dozen specimens occurred in a gathering. It appears, therefore, that many of the minutest inhabitants of the deep seek the surface at nightfall. Nor is this habit peculiar to animals affecting any peculiar habitat. Many of them are recognized as coming to the surface from bottoms of different depths, and of substances varying from the purest sand to the finest mud. It is interesting to observe how many of the small worms wriggle up to the surface, and contribute to the luminosity of the water; their presence being the more surprising, that they ascend from a depth of twenty or even fifty fathoms, and after being tossed all night by wind and tide, return to their dwellings at the bottom of the sea.

Although the animals captured in the surface-net include members of most families of the marine invertebrate fauna, it is the small entomostraca that swell the numbers, and, small as they are, the bulk also, of the creatures caught near the surface of the water at night; and by far the most abundant which came within the sphere of our observation belonged to the family *Diaptomidæ*, and the species *Temora Finmarchica*. This animal we invariably found during the day, as well as at night, although it prevailed chiefly after sunset; and by depressing the net, it was also taken at a depth of several feet under the surface. Under the shade of night, the rush of these animals into the distended net, held over the side or the stern of the boat, as it was slowly propelled through the water, was always accompanied by a vivid display of luminosity, which could never be witnessed without feelings of wonder and admiration. Such a spectacle we enjoyed one night in September, from sunset to sunrise; and then the capture was enormous,—far exceeding, in all probability, the number of the entire human family.*

The phosphorescence was observed to be remarkably persistent from the month of June till the end of September, with little difference in its apparent intensity, except what might be accounted for by darker or lighter nights. The degree of intensity varied in different places, at no great distance from each other, on the same night. This was very observable one night in September, when Mr. Robertson and his family circumnavigated the island of Bute, and especially in sailing through the Kyles. As a general rule, in calm weather, the luminosity is greater near the shore than at a distance from it. Occasionally, on a windy night, the whole expanse of the sea appears luminous, and every wave flashes with light as it breaks upon the shore. We may reasonably conclude, from the paucity of the smaller medusæ during the late season,

* It has been suggested that if every member of the human race had been occupied on the same night in the same way as ourselves, they would have produced no perceptible diminution in the microscopic life of the ocean.

that they play only an occasional and even a secondary part in the production of phosphorescence.

We satisfied ourselves that the luminosity of the individual is not continuous, but results from concussion or irritation, as when the water is struck, or the animals are agitated. In such circumstances small bead-like sparks often fell from the net, and shone for a second or two, disappearing with the exciting cause. All our observations on individual animals, or aggregates of them, showed that luminosity diminished in intensity after the first action, till at length it could scarcely be excited by any amount of agitation. The appearance of continuity in the light (apart from its own diffusiveness), caused by bodies moving in the water, is due in a considerable degree to the effect of the persistency of the image on the retina. The effect was always modified by the greater or less rapidity with which the net was dragged through the water.

There was no evidence that the light proceeded in any degree from the presence of dead animal matter. The occurrence of small dead marine animals on the surface of the sea is comparatively rare; and supposing such matter to be luminous, its effect would be quite imperceptible. It may be questioned, however, whether matter exuded from the bodies of the more common of the luminous animals may not contribute to the general effect; because diffused and lively phosphorescence could be produced by passing the finger along the surface of the muslin net, after the animals seemed to have been washed out of it; or may this effect have been due to the presence of other animalcules, which could not be detected even by the aid of the microscope?

All marine animals are probably endowed, in a certain degree, with the power of emitting light; the smaller animals exhibiting the property more than the larger tenants of the deep. Thus the phenomenon may be produced in different parts of the ocean by different animals, which will account for the contrariety of some of the observations of authors previously cited. But as a general result of our observations last autumn, we are satisfied that by far the greatest amount of the phosphorescence witnessed in that part of the estuary of the Clyde where we conducted our nocturnal researches was produced by two members of the family *Diaptomidæ*, namely, *Temora Finmarchica* and *Anomalocera Patersonii;* and one member of the allied family *Cetochilidæ*, namely, *Cetochilus septentrionalis.*

It is remarkable that, so far as can be inferred from observations made on species of entomostraca inhabiting fresh water, in ponds and rivers, these animals seem to be destitute of the power of producing luminosity. This is an aspect of the subject deserving of special investigation.

Dr. Bryce desiderated further investigation into the cause of the phosphorescence, and thought that the opinion of Quatrefages as to its being of the nature of electricity, pointed in a direction where the true cause might yet be found.

Dr. Thomas Anderson considered the paper to be peculiarly fitted for our Society, by its not only giving the results of observations, but by furnishing information as to the previous state of the inquiry, so as to enable them to form a judgment of these results. We are still left in the dark as to the emission of phosphorescence by marine animals; and we are too apt to take refuge in electricity, when in many instances luminosity is due to causes with which electricity has nothing to do. There are, for instance, some bodies which have the faculty of taking in light when exposed to the sun's rays, and of giving it off again when placed in the dark. The subject is one of great difficulty; and much research is required before we can hope to reach a satisfactory conclusion as to the cause of the phenomenon. There are other aspects of the sea, such as the red colour seen in particular tracts of the ocean, caused by the presence of multitudes of minute animals, which have yet to be investigated. The whole subject of marine life is one of great extent and interest, and, although surrounded with difficulty, holds out powerful attractions to the observer.

Mr Hart mentioned, as illustrating the depth to which luminosity descends, that he had often seen the light extending along the entire length of a rope attached to a boat, from the surface of the water to the anchor at the bottom, the light being excited by the vibration of the rope.

Mr. J. R. Napier and Mr. E. Hunt referred to different appearances assumed by the phosphorescence in peculiar circumstances.

Dr. Machattie and Mr. Mayer were disposed to attribute luminosity to frictional electricity and chemical action upon decaying animal matter.

III.—*On Petroleum.* By Professor Henry D. Rogers, LL.D., F.R.S.

Communicated from Boston, Massachusetts, United States, 16th November, and read to the Society, 13th December, 1865.

I propose to lay before the Society a synopsis of the leading facts and scientific views I have been able to arrive at concerning the mineral oil, petroleum, and its accompaniments, after not a little systematic and patient study of them.

Definitions.—Petroleum, using the term in its popular sense, means almost any kind of native rock oil, or so-called mineral oil. It is not a body of very definite chemical constitution, but consists mainly of the two well-known ultimate elements, hydrogen and carbon, united in proportions varying from C_{18} H_{20} to C_{26} H_{28}, and having commonly associated with these a minute amount of oxygen and nitrogen. The name, therefore, applies properly to a whole group of liquid, inflammable hydrocarbons, containing some of the gaseous and some of the solid hydrocarbons in solution. Petroleums are, in fact, bitumens possessing more or less of the volatile *naphthas*, and of the partially fixed semi-solid *asphalts*, or mineral tars. In its natural condition petroleum is usually a dark-brownish liquid, not pleasant to the sight, and even repulsive to the smell and touch. In its diverse forms it varies in colour from the tint of dark amber, through light-green to dark olive-green, and even to a hue almost black; and again, it graduates, in what we may call its mechanical *texture*, from the freest liquidity to a semi-pasty thickness. Owing to all these diversities, it possesses a very remarkable range of useful chemical and mechanical attributes. When "refined" or distilled, it is a beautiful, transparent, illuminating oil. Some of its very volatile products, rich in hydrogen, are very heat-engendering, while others, more abounding in carbon, are highly light-giving. While in some conditions it is especially suited for the manufacture of illuminating gas, certain of its products, on the other hand, afford the dyer or colourist agents for the production of a number of the most attractive tints that can fascinate the human eye. Indeed, through the refinements of chemical skill, there would seem to be almost no limit to the colour-imparting capacities of the wonderful distillates of petroleum. Another very valuable attribute of this substance, under those forms which are viscid and semi-unctuous, is its power as an efficient lubricator of machinery. Indeed, certain kinds are to be regarded as so much pure *native or mineral grease*, rivalling or surpassing most of the finest vegetable and animal oils and fats in their capacity for lessening friction. All these natural hydrocarbons agree in the one attribute of being more or less combustible, and some of them are very highly so. The vapours of the more volatile of these oils are, when mixed with certain proportions of atmospheric air, dangerously explosible; but the heavier mineral oils are less ready to emit any vapours, and those they do evolve will not catch fire nor engender explosive mixtures.

The constituent hydrocarbons in petroleum take on the gaseous or vaporous condition at very diverse temperatures. One very volatile product boils at 90° Fahr.; another at 91° Fahr.; a third at 92°

Fahr., &c.; but these are now known to be mixtures of two or more volatile hydrocarbons. The most volatile of all cannot be kept liquid but at a temperature below the freezing point of water, 32° Fahr. The most fixed of the hydrocarbons—the solid crystalline substance, paraffine—on the contrary, does not melt until it is warmed up to the temperature of 110° Fahr.

The specific gravity of the very volatile distilled oils is lighter than that of ether, while that of the solid paraffine is 0·870. Measured by hydrometer, the specific gravities of the petroleums as a group vary from 55°, the lightest, to 25°, or even to 20°, the heaviest.

Distribution of Petroleum in Depth.—Multiplied observations, extending over all the oil region including Western Pennsylvania, Western Virginia, South-eastern Ohio, and Eastern Kentucky, lead to the following interesting generalizations respecting distribution of the several kinds of petroleum, in the important element of *relative depth*:—As a rule, the lighter, more volatile, occur at greater depths than the denser and less evaporable. Another fact, attributable perhaps to the same cause, is, that these lighter oils evince more inclination than the denser ones to spontaneous flow from the strata whence they rise. Hence those "oil wells" which are *shallow* are in the main "pumping wells;" while—and it is a fortunate state of things—those which are deep are either self-gushing outflows, or, if not actually "flowing wells," are so nearly such as much to facilitate the pumping process.

At a first glance at the facts now stated—namely, that the deeper-seated petroleums are the lightest, most volatile, and most addicted to spontaneous flowing, and the denser ones not so outpouring—the uninitiated inquirer may feel perplexed for a rational cause; but the difficulty disappears so soon as he reflects upon the very different conditions to which the two kinds of petroleum are subjected within the rocky strata, the interstices, pores, and crevices of which they partially fill. It is an admitted general fact, that the rocky crust of our earth everywhere grows warmer *inwards*, at the rapid rate of about 1° Fahr. for each 55 or 60 feet after we pass the very shallow depth of some 70 feet, beneath which the sun's seasonal influence is imperceptible. Equally obvious must be this other fact, that all the deeper-seated vaporizable materials, such as the easily evaporated lighter petroleums, the ether-like mineral naphthas, and the gaseous hydrocarbons evolvable from these, must, without exception, exist in the lower warmer strata in conditions of very high elastic tension, or aptitude to expand. These compressed deeper-seated materials are also under a closer imprisonment than the more superficial ones, from the greater pressure of the incumbent very weighty strata resting over them. From both

sources, the higher heat and the greater pressure, they are in a state of far more compression than the heavier petroleums, already refined and distilled upward into the cooler and less-pressed-upon rocks nearer the surface. Thus we see why these upper heavier petroleums are what they are;—they were permitted, long ago, to part with nearly all their elastic gases and their readily vaporized lighter oils. In other words, the surface oils have long since *gone through* the sort of distillation which is *even now in progress* in the deeper, warmer repositories of the great bitumen brewery. To carry on my simile, the malt of the brewery has in the upper cooler strata altogether ceased its naphtha and gas-making fermentation; whereas, lower down, where the vats are warmer, the fermentation is in full activity. To this difference in the circumstances we may attribute, I think, the contrasts between the two classes of petroleum wells,—those which show energetic chemical and mechanical phenomena, such as copious spontaneous outflowing of the oil, with vehement and noisy blowings of the pent-up gases, and those, on the other hand, which display a sluggishness demanding the aid of incessant pumping.

Returning from this short speculative excursion,—explanatory, I think, of the facts above given, and of others yet to be narrated,—I proceed to sundry other statements which will be found to be more or less intimately connected with the very important question of the steadiness and *future durability of supply* of the mineral oils west of the Appalachian mountains of the United States.

General Limits, Geographically, of the United States Oil Regions.—All the districts where petroleum has hitherto been developed in any practically important abundance are situated to the north-west of the Great Appalachian Mountains. Indeed, no rock oil in notable quantity has yet been discovered anywhere to the south-east of these familiarly called Alleghanies.

The metropolis, if I may so speak, of the petroleum countries of the west, is an oblong tract in North-west Pennsylvania, following somewhat the general direction of the Alleghany River, and including portions of M'Kean, Warren, Forest, Venango, Crawford, Mercer, Lawrence, Butler, and Armstrong counties; but by far the most productive tract of this district is an area ranging from the centre of Warren to the Western side of Venango, embracing the Alleghany River and its Northern and Southern tributaries. The valley of Oil Creek, and those of the streams adjacent, may be termed the stronghold, so far as we yet know, of the petroleum of North America. In the State of Ohio petroleum has been found, and in some localities copiously, in the counties Noble, Adams, Franklin, Medina, Lorain,

Cayahoga, Trumbull, and Mahoning. In Western Virginia it occurs within the coal region in a long belt of country stretching from Tyler County to the Great Kenawha River at Charleston, over a mean breadth of about 30 miles, and a length of not less than 80 miles. Indications of it, implying some abundance, are likewise extensively met with in Eastern Kentucky, the most promising being perhaps in the valley of the Sandy River and its feeders. Petroleum is found in some districts in Western New York; also in Western Canada, in the valley of the Thames, near Delaware village; and it has been discovered in Michigan, Indiana, Missouri, and Tennessee. An inspection of my geological map of North America, contributed to *Johnston's Physical Atlas*, will indicate that the petroleum-bearing strata east of the Mississippi underlie a geographical area of more than 200,000 square miles; but, as I shall show in this or a future essay, we must not therefore infer that the chemical and physical conditions are everywhere throughout this wide territory conducive to the economical production of rock oil and its associates.

Classification of the Mineral Oils of Western Pennsylvania, and the Depths at which they are reached.

1. *Very Light Oils.*—These petroleums yield the minimum amount of kerosene, or illuminating oil, and are therefore of least commercial value. They are produced copiously, and in some localities very profusely, on Oil Creek and Pithole Creek, in Venango, and other adjacent districts in Western Pennsylvania; also, in the "flowing wells" of Western Virginia. The *depth* of the wells yielding these "light oils" is usually from 500 to 800 feet.

2. *Medium Oils.*—Rich or productive in illuminating oil, and therefore much valued for such. They range in specific gravity, as measured by Baume's hydrometer, from 32° to 40°. In Western Pennsylvania their most noted localities are certain wells on French Creek and its tributaries, and along the Alleghany River from Tionista, down even as far as Freeport, including sundry places in the vicinity of Franklin. A good amber-coloured medium oil occurs on Clarion Creek, one of the tributaries of the Alleghany River.

The *depth* of the medium oil wells is commonly between 350 and 500 feet.

3. *Lubricating Oils.*—These denser oils, having a prevailing specific gravity of 25° Baume, or a little lighter, are no longer termed "*lubricating*" when the specific gravity passes 32°; for then, as I have just noted, the petroleum is a medium or kerosene-producing oil, or No. 2 of our list.

The general *depth* of the lubricating oil wells, called familiarly the "*shallow wells*," ranges from about 100 to 350 feet, passing below which the well will tend to produce a portion of "medium oil." This lubricating oil is a more or less thick and distinctly greasy variety of petroleum, often admirably well adapted in its natural or unprepared condition for greasing the moving surfaces of machinery. It is proper here to remark, however, that both of the other two kinds, Nos. 1 and 2, will yield, under suitable chemical treatment, an unctuous lubricating oil well fitted for reducing friction.

The best-known Pennsylvanian localities for the heavy lubricating oil are some wells on the Alleghany River; about Franklin similar wells are common, and are said to be copious in this oil, on French Creek and its branches, especially Sugar Creek. This variety of oil occurs also in Western Virginia, in the counties of Wist, Roane, Ritchie, and Pleasants. The wells north of the Baltimore and Ohio Railway yield chiefly the heavy lubricating oil, and the oil from one well there has the consistency of "soft soap." In Ohio, also, these lubricating oils abound, especially in the "Mecca district," now a noted place for them.

The Geological Relations of the above-described three kinds of petroleum now demand a passing mention.

In the Oil Creek district of North-west Pennsylvania, the augur or chisel used in making the "Artesian borings" in search of petroleum generally encounters the "*first sandstone*" at a depth below the "superficial drift" of from 100 to 250 feet. This first sandstone itself has commonly a thickness of 40 or 50 feet. Beneath it again the instrument cuts through a mass of loosely united clayey rock or "*shale*," varying in thickness between 100 and 150 feet. It is just below the bottom of the sandstone, or in the upper part of this shale, that the borer generally develops the first intimations of the heavier or lubricating oils. Hence the usual shallowness here of the wells supplying this variety of petroleum.

2. The *second sandstone* underlies immediately the above-named shale, whose thickness of 100 or 150 feet separates the two sandstones. Thus the prevailing depth of the upper surface of the second sandstone is commonly between 240 and 450 feet, the sums of the thicknesses of the strata penetrated.

This second sandstone itself usually equals the first sandstone in its thickness, or is 40 or 50 feet. By a curious coincidence, there underlies this second sandstone a second shale, identical in thickness with the first shale, or varying in its diameter from less than 100 to 150 feet.

Now, it is a fact general throughout this oil district, that the medium, or illuminating petroleum, or the variety No. 2, first appears in the second sandstone, but more commonly and copiously in the shales directly under it.

3. A *third sandstone* is next reached, just at the bottom of the second shale; and this third arenaceous bed has a thickness in some places of 30, in other of 60, feet. To preserve this alternation of sandy and shaly or clayey rocks, there comes next a *third* mass of shale, directly underneath the third sandstone, but somewhat variable both in its composition and its dimensions.

Precisely as in the instances of the heavy medium oils, or varieties Nos. 3 and 2, the light oil, No. 1, is usually encountered upon penetrating the sandstone—here, sandstone No. 3—or else the mass of shale or clayey material which directly underlies it.

It is a general fact, too, that the *productiveness of the wells* is least with the shallow ones, greatest with the deepest, and intermediate in degree with those which are of intermediate profundity. In a wider sense, the nearer any oil well in its *locality* and *depth* approaches the conditions of a "*flowing well,*" the more productive, transiently at least, it is likely to prove.

It will be seen, by adding together the above-noted depths, that the total thickness of the body of strata perforated for the petroleums in Western Pennsylvania is not more, on an average, than about 550 feet. How inconsiderable a cover to so much mineral wealth, especially when we reflect that in two stages of its thickness this cover itself contains an important portion of the accessible oily riches underneath the soil! In some instances, but not in very many, the oil-yielding borings exceed 700 feet in depth.

Individual Peculiarities and Irregularities in the Western Oil Wells.

Some wells pump very little oil, but oil alone. Some very little oil, with much water, and the water *salt.*

Some wells, again, pump oil for only a few hours per diem, either as oil alone, or oil with water; and these producing hours are sometimes at irregular interrupted intervals; but at other times the "pumping period" is one particular time in the twenty-four hours.

Other wells supply oil—either oil alone, or oil with water—uninterruptedly day and night, with an unvarying rate of yield.

Some wells admit of suspension of pumping at nights or on Sundays without detriment; while others require a continuous and active pumping to keep down the water.

Many wells exhibit no disposition to any "spontaneous blowing" of the oil, and only yield it when pumped; while others, on the contrary, require no pumping whatever, but give forth, or "flow" the oil continuously. This outflow varies, between different wells and in the same wells, from a mere feeble dribbling to a gushing out of a column, often 2 inches in diameter, with prodigious force. It is an interesting and not uncommon fact, but one easily accounted for when we consider the conditions under which the petroleum and the gases are diffused within the strata, that the mineral oil is not always got *at once*, many of the best wells requiring to be pumped for days and weeks before they begin to flow.

Besides the two main classes of wells—the pumping and the spontaneously flowing ones—there is still a *third class*,—those which are a little disposed to flow, or that may be said to be just on the point of spontaneous emission—which are materially assisted by pumping—the effect of which is partly to *lift* the water and oil, and partly to produce an *agitation* that helps to dislodge the vapour of the oil, and thus assist in lifting the column by the upward pressure of the bubbles of buoyant gas.

Some wells, curiously enough, flow much oil and water, and very *little* gas; others very little oil and water, and a great amount of gas; indeed, there are those which discharge gas only, and are therefore familiarly called the "*dry-blowers*."

So copious is this outflow of the inflammable hydrocarbon gas from many of the Artesian wells—sunk formerly only in quest of "salt water," and now more recently in search of petroleum—that inventive superintendents have not unfrequently turned this easily managed combustible under their "salt pans," or the boilers of their steam engines, and converted it into a most efficient fuel. The *first*, or one of the first, successful applications of native gas to this use was achieved some twenty-five years ago at one of the salt wells in Western Virginia, near Saltville, on the Kenawha, where the abundantly out-flowing gas was diverted into flues, and made to burn underneath long rows or banks of the large hemispherical brine basins, or evaporating "salt pans." In the Pennsylvanian petroleum region the first similar utilization of the gas from the salt wells was accomplished by the superintendent of the "Lady's Well," sunk in 1861. Since then there have been, or are still active, some seven or eight oil wells, scattered somewhat widely, which are noted for thus using their own gas as fuel for propelling their pumping steam engines. On Oil Creek one of the "Hatch Wells" has been yielding gas enough to supply *eight engines;* and one of the borings at Economy Wells, near

Tideoute, Upper Alleghany River, has, though it is only 145 feet deep, been producing, besides a fair amount of oil, enough of gas to keep two engines running.

In the midst of this Western oil region there are many Artesian borings which yield no oil at all, but which discharge an abundance of water, propelled upward by the gas; and this water is commonly very salt, containing from five to ten per cent. of pure table salt.

Irregularities.—There is as striking a diversity in the oil wells in their *regularity* of yield as in their respective total productiveness. Some of them flow steadily day and night, with little or no variation; while others suspend their flow for a part of the day, and resume it for a definite interval. Some will be tranquil for a few hours, or a part of an hour, and then flow for minutes or hours, and will maintain this curious alternation with great accuracy, both as to the intervals and the durations of the productive outflows. Certain wells flow once in half an hour; others once only, and for a short interval, in several days. As one among many of the instances of this fickleness, I may cite the so-called "Yankee Well," or Cherry Run, in Pennsylvania, sunk in 1864 to a depth of 606 feet. After being pumped for two weeks, it began to yield, by this process, from ten to twenty barrels of oil per day; but the workmen commencing to pull up the tubing in the hope of improving it, the oil, to their surprise, began to flow spontaneously at the brisker rate of thirty-five barrels per day, increasing after a little while to fifty barrels. In this well the flow was *spasmodic*, usually continuing from five to seven minutes, and then ceasing for about twenty minutes, and these intervals of alternate flow and respite seldom varied more than one or two minutes.

"*Lifetime*" *of the Oil Wells.*—It is not to be doubted that every oil well has a "lifetime," or limit to the duration of its productiveness; although in numerous cases the rate of decreasement of yield may not be marked, or even very perceptible, within days, or indeed weeks; though a comparison at intervals of months will almost invariably demonstrate a *decided declension*. Sometimes, in fact, this reduction of supply is very abrupt, and wells, previously active, *cease altogether*, as if overtaken by some catastrophe. As a general rule, this rate of declension is noticed in the pumping wells to be *slower* in proportion as they yield little,—or, in other words, the *less copious* are the *more permanent*. Furthermore, it is notorious that a "flowing well," starting with say a yield of 200 barrels per day, will generally fall off in its product to the extent of twenty barrels in a month's time; while "pumping wells," that in their outset, three years ago, afforded only three barrels daily, will have diminished but little within all that period.

Of the "flowing wells" of the whole oil region there is *not one* which approaches in yield *now* the product it gave six months ago. The more copious flowing wells have generally ceased their flow after twenty-five or thirty months; but a few of them have prolonged their supply, *though at a much reduced rate*, to the limit of three years from starting. Some very recently published interesting statements have appeared in the *Boston Daily Advertiser*, which confirm this general rule. Writing from North-west Pennsylvania, the narrator mentions that the "Buckeye Well," not far from Titusville, commenced to flow, in 1861, 800 barrels per day, and it now yields only about 8 barrels; also, that the well-known "Empire Well" struck oil in 1861, and flowed the prodigious outpour of 3,000 barrels per day, and it now "*pumps*" only 60 barrels. He adds that the "Ocean Well" struck oil six weeks ago, and is flowing 300 barrels per diem.

It is evident, from the facts here stated, that our anticipations of a sustained future supply of petroleum must be based, not on the wells now yielding or active, but upon new explorations, and on the daily addition of fresh exits for the oil, with constantly improving appliances for economically reaching and securing it.

The suddenness of cessation above alluded to is well illustrated in the history of the "Empire," the "Coquet," and the "Jersey" wells. Each of these supplied from 800 barrels per diem downwards, and each abruptly suspended its flow; but it would have been an error to infer from these ceasings that the oil had become exhausted,—for these very wells, and many other similar ones, have, when pumped or otherwise assisted, been made to yield a large supply, though never equal to that they gave when they were spontaneously flowing.

It may serve to elucidate the nature and the causes of this startling stoppage of production in the "oil wells," to bear in mind, that it is not so much the *exhaustion of the petroleum itself* which causes the wells to cease their yield, *as the exhaustion of the pent-in and compressed gas*, for it is mainly to the elastic tension of this latter substance that we are to attribute the cessation of the oozing forth of the liquid oil.

Present General Yield in Petroleum of the Venango Oil Region.—According to the candid and careful newspaper correspondent already quoted, the Pithole Creek district alone (a limited vicinity) is now producing 5,000 barrels per day; and it is estimated that the aggregate present yield of Venango County amounts to at least 13,000 barrels, which is at a rate of more than 4,000,000 a-year. This authority states, that although diminishing at some points, the yield starts afresh at others, and on the whole is increasing. Mr. W. Wright, in a chapter on the "Statistics of Production," in his little volume on the *Oil*

Regions of Pennsylvania, more than confirms the above; for in an interesting summary of the wells in operation, the total yield in barrels of each group or locality, and the average daily yield of each neighbourhood, I find the grand totals to be thus:—

Wells in Operation, ...	322
Total Yield in Barrels, ...	8850
Average Product per Day, ...	75·5

Surface Indications of Subterranean Petroleum.—There can, of course, be no external sign so sure of the presence or nearness underground of a rich general store of mineral oil as the actual visible exudation of it, such as has been witnessed for more than a century past on Oil Creek and some other known western localities. Such constant spontaneous escape at the surface testifies, not only to a greater or less saturation of the strata below, and of their joints and crevices, but also equally clearly to the permanency of the conditions, *within* these strata, which tend to distil ever-fresh supplies upwards towards, and even to, the soil. To such ocular tokens of the proximity of petroleum, appealing year after year to the inhabitants of the oil regions, is due that confidence of success with which the oil explorers in all the North American petroleum districts have probed and penetrated the strata for this prized material.

A less positive, though still a very significant indication of proximity to petroleum, is the slow emission or copious outgushing of hydrocarbon gases and vapours, especially in the native springs of the country, or in the Artesian wells, bringing the briny water of the rocks towards, or actually up to the surface. Both of these sources of subterranean rock oil, the "burning springs," so called, and the "salt wells," display, even where the gas is not accompanied by any very obvious amount of the liquid oil,—which elsewhere does attend it,—so steady an effusion of the aërial hydrocarbons, that no geologist, or even less initiated observer, can resist the persuasion that they betoken a near vicinity to the other class of hydrocarbons, the liquid petroleums.

Statistics of the Annual Product and Money Value of the Petroleum.—To give more completeness to this descriptive part of my communication, I will append here some concise statistical statements of the rate of yield of the oil wells, the more marked fluctuations in the market price of petroleum, and the best authentic estimates of the present annual value of this important element in the native mineral wealth of the United States.

The finding and extraction of petroleum in the oil region of Pennsylvania, as a commercial business, commenced no longer ago than 1860, but the boring operations were not very successful until 1861.

The first Artesian well sunk expressly for the mineral oil was made in 1859, near Titusville, on Oil Creek, close to the county limit between Venango and Crawford. This boring yielded some 8 barrels per day, enough to incite to further similar enterprises, so that, in the summer of 1861, there were already, in the valley of Oil Creek, a number of prosperous flowing wells, yielding in the month of August some 2,500 barrels daily. In the spring of 1863 the product had become so augmented that the price of crude oil fell from 25 cents per gallon to less than 1 cent (or a halfpenny). The year 1863 witnessed very fluctuating, but in the main rising prices, and throughout the year 1864 this augmentation in the market value continued, and with a more even acceleration, until at last, about the beginning of 1865, the crude oil sold at the wells at the high rate of 13·50 dollars per barrel, or 34 cents per gallon. In April of the present year, 1865, prices had so receded, that illuminating oil, at the wells, sold for only 3 dollars per barrel, but since that date the price has been gradually ascending again. Crude petroleum is at present, in New York prices current, at 60 cents per gallon. Good authorities estimate the *average* past prices of crude petroleum at the wells to have been between 6 and 7 dollars per barrel; and Mr. Wright, a careful observer, computes this year's product of the crude oil at a commercial value of 25,000,000 dollars; and adding the enhancement of value conferred on it by refining, barreling, transporting, and storing it in commercial centres, he counts the year's receipts from the oil business, at its present stage of growth, at between 30,000,000 and 35,000,000 dollars, or about £7,000,000 sterling. The Philadelphia Board of Trade states, in its annual report for 1864, that the petroleum of Pennsylvania for the year had a value of 46,912,430 dollars, or more than £9,000,000.

Let me here recall attention to the fact, that this prodigious accession to the annual productive income of this country is very nearly all of it derived from an area of only 400 or 500 square miles, equivalent to a square plot of but little more than 20 *miles in diameter*.

I hope, gentlemen, to avail myself of some future fit occasion, to submit to you my theory of the geological origin and conditions productive of the petroleum now exuding in such abundance through the Artesian wells of the United States oil regions. Meanwhile, permit me to recall to your notice what I put in print upon this topic in May, 1863, in the magazine called *Good Words*. You will there find a general synopsis of my views, but I trust to be able, at a not remote future day, to apply the generalizations there adventured to the elucidation of most of the phenomena descriptively set before you in the present essay.

I will close this introductory paper with simply remarking that the *geological* signs of petroleum in the American oil regions are, chiefly, certain specific characters in the aspect and *composition* of the strata; the manner in which *these* underlie the surface, and the *depths* at which the supposed oil-producing rocks may be ascertained to lie; or in other words, the symptoms are—the chemical and mechanical nature—the structure or mode of arrangement—and the depths or degrees of natural warmth of the strata, adapting them to engender, to hold within them, and when opened to send forth, a more or less steady efflux of the pent-in petroleum.

DR. THOMAS ANDERSON observed that amongst the interesting points in the paper was the great extent over which the petroleum was spread. It was interesting also to notice the condition in which it was found, passing from the fluid to a semi-solid state, till it became like soft soap. In this latter condition it resembled that which was found at Rangoon, which was imported into this country, and worked up by Price's Candle Company. In one respect, however, it differed from the substance found in Burmah, for there it was only necessary to dig a hole a few feet deep to have it immediately filled with petroleum, which at the temperature of that warm climate is fluid, although when it is brought here it has the consistency of soft soap. This kind of petroleum was further of interest, as it was in it that Christison discovered paraffine as a natural product. He applied to it the name of petroline, being then ignorant of its previous discovery by Reichenbach among the products of destructive distillation. In speculating about the origin of petroleum, the general idea was that it was produced by the distillation of coal by subterranean heat. Whether that view is correct or not, it is a remarkable fact, that in some localities far distant from coal-fields, the same substance is found, and in these cases a different explanation is required for its origin. In our own country, petroleum occurs in sandstones overlying the coal, and a century since, petroleum was distilled from a sandstone in Shropshire, so that the fact of the distillation of this substance is not so very recent as it is sometimes supposed to be.

MR. WÜNSCH stated that in the neighbourhood of the city there are sandstones saturated with oil, and some which, from the oil diffused through them, change their colour when exposed to heat. The oil now abundantly produced by the distillation of shale in Scotland, indicates the existence of the substance on an extensive scale. The geological conditions of the Scottish coal-fields bear a considerable similarity to those of the American coal-fields, as described by Professor Rogers, particu-

larly in regard to the abundance of anthracite. In these circumstances, it was not unreasonable to hope that oil might yet be discovered in Scotland.

REV. MR. CROSSKEY mentioned that oil is found floating on water to the west of Edinburgh, where it can be collected in small quantities.

DR. T. ANDERSON observed that oil was actually produced in Derbyshire on a workable scale, and Mr. Young's success in producing it there, led him to undertake the distillation of the Boghead coal. There was no doubt, as Mr. Wünsch had stated, that some of our sandstones, such as some near Bishopbriggs, contain a considerable portion of oil. All these facts render it far from improbable that some fortunate individual may yet "strike oil" in Scotland as in America.

On the motion of DR. FRANCIS H. THOMSON, the President, the Society agreed to transmit its cordial thanks to PROFESSOR ROGERS for his valuable communication.

IV.—*On Electrically Impelled and Electrically Controlled Clocks.* By WILLIAM THOMSON, LL.D., F.R.S., Professor of Natural Philosophy in the University of Glasgow.

Read January 24, 1866.

PROFESSOR WILLIAM THOMSON exhibited and explained an improved arrangement of magnets for the electro-magnets and coil used in the Jones's system of controlling clocks electrically. The object aimed at in the system is to force the time of vibration of the controlled pendulum to agree with that of the controlling clock, and to interfere as little as possible with the extent of range through which the controlled pendulum vibrates. To produce such an effect the proper place for the controlling force to act is as near as possible to the ends of the range through which the pendulum swings. A force acting on a pendulum when it is near or passing through its middle position tends either to stop it or to make it vibrate more widely. Accordingly, the middle position is the proper place for the impelling force to act, by which the pendulum is kept vibrating; and this is what is put in practice in the dead-beat escapement, also in Bain's electrical pendulum. Bain's arrangement of coil and magnets, which was introduced by him for the purpose of keeping a pendulum, and through it the works of a clock, vibrating by the energy of an electric current, proved to be a very convenient way of applying electro-magnetic force to a pendulum. It was

accordingly adopted by Mr. Jones for the very different purpose contemplated in his admirable invention, which was to regulate a clock kept going by its own weights, and compel it to agree in its rate with the distant controlling clock. In Bain's arrangement two bar-magnets are used, fixed in a horizontal line, with their north poles together. The pendulum carries at its lower end a coil or bobbin of silk-covered copper wire, with a wide aperture or core, through the centre of which the line of the magnets passes. The pendulum in vibrating carries this coil from one side of the double north pole to the other in every vibration, and the mutual action between the double pole and alternating electric currents through the coil gives rise to horizontal forces alternately in opposite directions, by which the pendulum is kept vibrating, while resisted by the air, friction in the clock-work, &c. The distant south poles may be regarded as practically inoperative, because of their comparatively great distance from the coil; but the small practically insensible forces which they produce are, however, at each instant opposed to the true resulting effect produced by the central north poles. Whichever way the current flows, this arrangement gives the maximum of force to the pendulum at the middle of its arc; but it gives enough of force, when the pendulum is near the ends of its range, to produce the desired controlling power, with very moderate and cheap electric force, as is proved by the success of Mr. Jones's plan, even without the improvements of arrangement which have been introduced in Glasgow. The first of these improvements consists in drawing out the magnets, and fixing them so that their north poles shall be just reached, or barely covered by the coil, as the pendulum swings to either end of its range. The effect of this is to increase the electro-magnetic force where it is wanted—that is, at the ends of the range—and to diminish it in the middle, where it is not only not wanted, but has sometimes been found in practice very detrimental. It must happen occasionally that by some accident, whether the telegraph wires be carried away by a storm, or some temporary stoppage or failure occurs in the controlling-clock-contacts or battery, the controlling current is interrupted for a time. During such an interruption the controlled clocks are simply left each to go at its own rate as an ordinary clock, and in the course of a few hours any one of them may gain or lose several seconds. When the current is re-established, they ought all immediately to submit again to the control, and go on each with the few seconds gain or loss it may have made while it was left to itself. But if the pendulum of any one of them chances to be vibrating on the whole against, or considerably more *against* than *with*, the re-established alternating electro-magnetic force, it may be stopped

because of the greatness of this force on the pendulum in or near its middle position; and after a time (several minutes in general) it might be started again in the proper direction by the timed impulses which it constantly receives. (This curious phenomenon was illustrated by experiments performed before the society). Accordingly, after accidental stoppages of the current, some of the controlled clocks, during the first few months of the Glasgow experiment, were more than once found going, but several minutes slow. After the magnets were drawn out, according to the theory explained above, no such derangement has ever been experienced; and the greatest derangement that a stoppage of the current has caused in any one of the controlled clocks seems to have been two seconds, four seconds, or six seconds. Such a derangement can mislead no one, as the University of Glasgow requires, as the sole condition under which it will supply electric time-signals in any case, that a galvanometer showing (by the lost deflection on the sixtieth second) the precise moment of the beginning of each minute shall be exhibited with every clock under control which has a seconds' hand; and no one who is so exact as to look to seconds will omit to verify by the galvanometer the time-signal he takes.

A further improvement consists in as it were bending the bar-magnets round into the horse-shoe shape, so as to bring their south poles from the positions in the axis of the coil where they do damage, mitigated only by the greatness of their distances from the field of action, to proper positions close outside the coil at each end of its swing, where they act forcibly at close quarters in aid of the useful action of the north poles when the coil is at either end of its range, and yet mitigate very effectively its detrimental action on the coil in the middle part of its swing. One of the many new clocks now in the course of construction by Messrs. Mitchell, to be controlled from Professor Grant's mean time clock in the University Observatory, had been so nearly completed, with the proper form of coil and disposition of horse-shoe magnets on the new plan as to allow the action to be exhibited to the society; but Professor Thomson had only had it to try for an hour before the meeting of the society. He hoped in the course of a few weeks to make experiments on it which would allow him to report to the Society—

1. The number of seconds per day of error in its uncontrolled rate from which a definitely specified amount of electric current would hold it controlled.

2. The greatest number of seconds per day of error which could be checked in it by any current incapable of stopping it by catching it the wrong way.

This improvement is even more important for the two seconds' pendulum of a turret clock than for the ordinary seconds' pendulum. After nearly a year of experiments and trials of coils and magnets more and more powerful, the $2\frac{1}{2}$ cwt. pendulum of St. George's Church clock had, by a proper disposition of merely bar-magnets, been brought under control in the same circuit as the other clocks, all of which have seconds' pendulums, without the necessity for a special wire and modified system of currents, as specified by Mr. Jones in his patent. It may be confidently expected that the new improvement now brought forward will render the control of other heavy two seconds' pendulums a much easier matter (that is to say, much more stringent with the same amount of electric coil); and it is to be hoped that the example first set in Scotland by the University of Glasgow, in putting its tower clock under control, will be followed not only in St. George's Church clock, but very soon in every public clock in Glasgow, Paisley, Greenock, and Ayr, and even in Edinburgh, notwithstanding the time gun.

V.—*On a Land Standard Electrometer, and a Marine and Land Telegraphic Testing Electrometer.* By William Thomson, LL.D., F.R.S., Professor of Natural Philosophy in the University of Glasgow.

Read January 24, 1866.

Professor William Thomson exhibited several new electrometers, for the construction of which he had been fortunate in having the skilful, patient, and always ready action of Mr. White, instrument-maker, of Glasgow. One of these, a large standard electrometer, is an admirable piece of workmanship. It has a micrometer screw of two inches range and 1·50-inch thread, cut by a very perfect Whitworth lathe in Mr. White's workshop, and is in all points thoroughly well executed. This instrument is on the same principle as the small portable electrometer shown first to the Philosophical Society about three years ago, of which the principle need not be re-explained now. It differs merely in using gravity (admissible in it as a stationary instrument for use on land) instead of the force of a spring, for balancing against the electric force to be measured, and in being of proper dimensions to give great accuracy and a very uniform scale through a wide range. The value of a division of the micrometer screw is in it equal to about one cell of Daniell's battery, and the effective scale is 80,000 of these divisions. It can be easily read within one or two divisions,

and with care it may be read with certainty of a fraction of a division. Thus it measures electric potentials from one cell upwards to 80,000.

Another of the electrometers was an improved electrometer of the "divided ring" class, of which several have been shown to the Society in the course of the last nine years. The new improvements which it includes have for their primary object to render it available not merely in a laboratory or meteorological observatory in the hands of scientific operatives, but in a factory or at sea. They had been long tried for, without decided success, but owing to the impulse afforded by the recent Atlantic expedition, had been satisfactorily planned, executed, and tested within the last three months. The same form of instrument could be made of different sensibilities, from a highest (exceeding any hitherto attained) to a proper degree for use on board ship during the laying of a cable. An electrometer at sea does not as yet *necessarily* supersede the marine galvanometer for the insulation test. But to reduce chances of waste, or even of failure, as low as proper foresight can reduce them, it is quite necessary that an electrometer be kept in use at the Valentia shore station during the operations of 1866. The 1865 Atlantic cable, thanks to steady and successful attention to the requisites for electric perfection in the Gutta Percha Company's Works, and to careful watching throughout the subsequent stages of the manufacture and shipment, insulates from fifty to a hundred times as well as the old Atlantic cable; but its insulation can still be satisfactorily tested at sea in the Great Eastern by the marine galvanometer. But a still better insulated cable, in a more rapidly rolling ship, can only be tested for insulation by an electrometer; and the improvement of this instrument into a thoroughly practical testing appliance for use at sea is an important object, now when we are in view of submarine telegraphic projects for every ocean, in comparison with which all hitherto completed will sink into insignificance. The marine electrometer now exhibited to the Society must be thoroughly proved at sea before it can be exclusively trusted for the insulation test in any such work as the laying of an Atlantic cable.

VI.—*A few Remarks on the Treatment of the Cattle Plague.* By Mr. Richard Brown.

Read February 7, 1866.

The subject of the following remarks is one of vast importance to the world—one in which all are alike concerned, the highest as well as the lowest. To the labouring classes cheap butcher meat is a matter of great consequence, and the want of good milk in families is a circumstance deeply to be regretted, seeing it is one of the most important articles of their diet: anything, therefore, that can be done or devised for the prevention or cure of this dread disease should be at once made known. It is with this desire I wish to ventilate my ideas on the subject, and if possible elicit the opinion of others. I am well aware that the most eminent men of our day have recommended a great variety of different kinds of treatment, medicinal and others, but so far all have hitherto failed. A very few cases have got better under treatment; and these, I am inclined to believe, have been cured more by gentle means and the restorative powers of nature than by anything else. I have carefully watched all the different theories and recommendations as they were brought forward, the *Times* newspaper having given daily long and elaborate articles on the subject. I have also watched with interest the different remedies which have been recommended and tried locally. We have had depletion with strong stimulants; bleeding with stimulants, astringents, and tonic remedies in such doses as to render it utterly impossible for the system to absorb them. We have had homœopathic treatment—a very safe system, and one in which I have some little faith: only I don't think the medicines given by the Homœopathists to be the proper ones, and were in rather too small doses to do good. We have had vapour baths—wet sheet packing, in combination with medicine—certainly capital things when properly applied, but dangerous and often fatal when used indiscriminately and without a proper knowledge of their powers.

I am not a medical man, but I have studied in that school called experience. I have also closely watched and carefully studied the Laws of nature. For years I was under the allopathists, and never knew what health was until I went through a short course of the hydropathic treatment; since when (now a good many years ago) I have enjoyed excellent health. I have since recommended the same treatment to many others, with the most beneficial results. I have also

carefully studied the various modes of treatment under this system, also the cause and effects of disease, and have come to the conclusion that *nature is the great restorer in all cases*, and the more gently we can assist its delicate operations the sooner we will effect a cure. My own personal experience goes to prove this; and I have also seen it proved in others.

These remarks are made to show that the assertions I bring forward are not made at random, but are the results of some little experience.

The conclusions I have come to in regard to the treatment of this disease—"the Rinderpest," or under whatever name it is called—are, that the remedies have not been adapted to its cure; that the methods of treatment were too complicated—I mean too many things were given or tried at once, without proper care being given to diet and the comfort of the animal while under treatment. Animals do not differ much from human beings in this respect: the latter, when any ailment affects them, and especially in such cases as fever, cholera, and the like, require the most constant attention. So with animals in such a disease as the present: they must have the same unremitting care ere the disease can be cured.

As far back as October last I wrote to the newspapers on the cure of this disease by the "Water Treatment," under the signature of these words. I was immediately followed by others on somewhat similar grounds, but in such a manner as showed they were not familiar, or even acquainted, with the therapeutic properties of water, especially when combined with medicine. The consequence has been that, so far as I am aware, their system has not succeeded generally; and consequently the water treatment has got a bad name.

Want of time and a railway accident have prevented me pushing the system I recommended as I would have liked; but I understand, in one or two cases where it has been tried, its success has been perfect. I have still the same confidence in its efficacy, as I find, where any cures have been effected, they have all approached my system as near as may be, especially as regards diet, which I call my negative system, the positive one consisting of active treatment.

I am therefore inclined to think that the system I advocate is the one best adapted and most likely to cure this disease in its true form, and to do it without the slightest injury to the animal, the great beauty of the treatment being, that if taken in time the cure will be speedy and certain, without almost any diminution of the animal's strength; whereas under the other system, should recovery take place, the animal is an invalid for many days after,—perhaps a week or month.

As I stated in my letter to the papers, I believe this disease to be caused by poison in the system, causing fever, inflammation, diarrhœa, and ultimately death. Nature in this case, as in all others, tries to cure; but the poison, if not irradicated, is the stronger, and consequently gains the victory. Now, what we have to do is to assist nature—relieve it of the poison, and the cure will very soon be the result.

Nothing that I know of—and in my experience I have seen a good many things tried—assists nature so well, and is so kindly to it when properly applied, as water. But herein lies the difficulty; hot baths, vapour ones especially, are capital things; but in most stages of this disease I don't think they are at all applicable—certainly not in its earliest ones, when there is a great amount of nervous irritation; then they only tend to aggravate. This I have proved in many cases.

My experience goes to prove as best, and what I recommend in this disease is, "wet sheet packing." To parties unacquainted with the water system, and particularly packing the body with wet sheets, they can hardly form an idea of its superior influences, and with what quickness it reduces fever, allays inflammation, and arrests diarrhœa. I have tried it again and again in all these cases, and have never known it fail.

If this disease is taken in time, whenever noticed, the chances are you have only one of these to cope with; but if allowed to go on for a time, it is almost a certainty you have all three; consequently the cure becomes more difficult and protracted; *and if the disease has been allowed to go too far, I believe it to be incurable:* hence the great importance of seizing the first symptoms of ill-health.

For the positive treatment take three or four large sheets or rugs—the latter are much the best; but when these are not at hand, sacks ripped up, carpets, and such like, come in as capital substitutes. Let them be as large as to go easily round the animal; take one, and dip it wholly in water—cold water; wring it out, but leave it still pretty wet, though not dripping. Roll this round the animal as closely as possible; then cover up tightly with the other dry sheets, one after another, the object being to exclude the air. These may be kept on by ropes, strings sewed on at different places, so as to tie them together, or strong pins. The wet sheet will get warm immediately. In about an hour, or at most an hour and a half, or whenever the animal is seen to get restless in them (which may be sooner than the hour, if the fever is very severe), take them off and rub the animal well all over. Re-dip the sheet, and apply as before, doing this three or four times, till the fever is allayed and the animal is seen to be recovering; upon which discontinue the process, and only renew it as appearances may

warrant. It is wholly in the *renewed* applications that benefit accrues. Should the packs be allowed to remain on longer than the above specified time, nervousness is excited and the fever increased, whereby a positive injury is done. If the disease has been attacked in its earliest stages, the improvement will soon begin to manifest itself in the animal—in fact, the first pack will show it; but if the disease has been allowed to get seated, the change for the better will only be apparent after renewed applications of the sheets.

While under the treatment the animal should have water to drink given to it frequently, not in large quantities at a time, but in sips, as it were. I lay particular stress on this, as I consider it to be an important element of the treatment, as it assists to a great extent the chemical action going on in the stomach, which has in this disease been found to be almost invariably loaded with food; and so long as this lies there undigested, there is no use of giving the animal more. Herein lies the negative part of the treatment. I say, therefore, we must under no consideration give it food until it is seen to be recovering, which is best indicated by its chewing its cud. Therefore, when this is the case, and the packs are discontinued, a little gruel should be given, to be increased very gradually from day to day as recovery is perfected; but it must be strongly borne in mind that, as the stomach and whole body are in a very weak state, the utmost care and caution must be exercised in this respect—in fact, quite as strictly as with a sick individual. So long as the nervous system is unstrung, there will be no digestion; but so soon as this is strengthened, which the packs will rapidly do, assisted by the water drinking, digestion will soon begin.

I am inclined to believe that very many animals die from inattention to diet in this disease; and if food in a great many cases was at once withheld from them, and nothing but water given them to drink, that they would recover.

It is a well-known fact that a little food well digested does good; but a great deal overloads the stomach, causing weakness and even disease itself. We often find the greatest eaters are the thinnest people; and so with animals: those in best condition are those that are good, but not over-eaters. Should, therefore, disease be prevalent, and the latter failing exist, it is simply adding fuel to the flame by encouraging it, and allowing the poison to take a stronger hold.

In regard to medicine, I believe in certain stages it may assist and do good; but on the whole I consider that it is only useful in this disease when the animal is costive—when a slight aperient may be given, such as treacle or oil. To the former may be added, or given subsequently, about half an ounce of chlorate of potass. These, how-

ever, or any other medicine, should never be given along with the packs—I mean both together—as their energies will be wasted in withdrawing it from the system, and the effects of both rendered inoperative; besides, when both are given at the same time, the medicine tends to sicken the animal. Medicine, to be of use, should be given some hours *before* applying the packs, or immediately *after* they have been applied. I would much prefer, and would recommend the latter (that is to say, if medicine is to be given at all); but the chances are, it would not be required if the packs were tried first,—as, strange anomaly as it may appear, they have the effect of not only stopping a too violent action of the bowels, but also of opening them when bound up, but in so natural a way as not to reduce the strength, which cannot be done by medicine.

Strong stimulants or astringent medicines should on no consideration be administered, as it is almost certain they will injure, and prevent a cure. Fever and inflammation being present, they are both perfectly inadmissible; in fact, in almost every case I have read of where they were used the results have been fatal.

I understand it is very common to deplete, and then use strong stimulants to keep up the system; but such a system is altogether foreign to nature, and one that I cannot see any argument for. I can understand how that a little stimulus, given in severe cases of prostration, will be very beneficial; but, as I said before, I consider in this disease it is hurtful.

Great care and attention should be paid to the comfort of the animals; the house they are kept in should be dry, well ventilated, and warm; they should also have plenty of dry clean straw about them; and so soon as the packs are taken off, a large dry rug should be put over them; and it is almost a certainty that this will have the effect of bringing about a free and copious perspiration, showing that nature is relieving itself by the skin. When this is so, it may be allowed to go on for some little time, when the animal should be again rubbed well all over, and the rug thrown over it afterwards, still giving a little water to drink. This perspiration is one of the best signs of recovery, as nature is thus showing it has got the ascendency over the poison, and is asserting its sway, as it were, by thus throwing it off.

The greatest care in giving food in this stage should be exercised, as the slightest overfeeding is sure to bring on a relapse; and if so, the packing must begin again *de novo*.

I have said that sweating either by vapour or other means is not applicable in this disease; but it must not be assumed from that, that I am against sweating in certain stages of it; but it is so very difficult to

be able to tell correctly with an animal when it is the proper time to apply and to withhold it, that I recommend it should not be used. Sweating by forced means on a loaded stomach is one of the most dangerous things that can be done; and I have shown that such is often the case with animals in this disease. It is, however, very powerful in cases of obstructions, congested liver, and the like; but it must not be supposed because there is free perspiration a cure will follow. If in this disease the effort is not free and spontaneous, the treatment will be too taxing on the nervous system, and it will be stirred up beyond its power to make efforts to throw off the disease. Forcing, therefore, won't do; a little coaxing must be adopted; the nervous system must be gradually re-established. Hence the wet sheet packing is the most soothing application that can be applied to the system; and any one present who has tried it will, I am sure, bear me out in what I assert. It is also one of the safest, most effectual, and powerful means of cure of the water treatment. It is in fact similar to a vast poultice applied all over the body—only it has not the disagreeable feeling such a poultice would have—and there is no difficulty whatever in applying it. It carries off heat, and this heat is applied in converting the moisture of the wet sheet into vapour, so that the animal may be said to be in a vapour bath of its own making. This is often mistaken for perspiration, which is an error, as the sheets are not allowed to remain long enough on to induce that. It will therefore be seen that the frequent changing of the sheets carries off the heat from the surface of the body; the internal organs are relieved; the pulse being reduced, and the skin placed in a favourable state for perspiration. This last generally follows, and nature relieves itself. Should this not take place soon, and the skin become dry, the packing must be recommenced, leaving an interval between each, to see its effects. When the sheets are allowed to remain on too long they become a stimulant, and actually irritate, when it was intended they should soothe. In cases of severe fever the heat accumulates so fast that they would require to be changed every quarter of an hour, otherwise they would be so irritating as to have a very prejudicial effect.

Disinfectants should be freely and liberally used not only in the byre and outhouses, but in all places about the buildings where the animals may be going about. Chloride of lime is a cheap and useful disinfectant. Peat moss made into charcoal is also very good; and as this can be had for very little in many districts, it should be liberally used. It at the same time makes capital manure. As I said before, every means of prevention should be adopted, and the same care and

attention bestowed on the animals as are given to individuals who may be labouring under fever and the like.

I may sum up my means of prevention and cure as follows:—Isolation—disinfectants—attacking the disease in its first stages—simple treatment—with care and attention in diet and otherwise.

As this disease is contagious, I am thoroughly of opinion that it is imprudent in the highest degree to allow any person whatever who has been near an infected animal to go near others who may be healthy, or about whom there may be a report of disease. In this latter case I would recommend that any animal seized with illness, no matter what it is, be at once separated from the healthy ones, and put in a place by itself, a good distance away from the others; and if a veterinary inspector be called in, that he may see the animal there, and there only. The party who accompanies him should not be allowed to go in amongst the healthy ones afterwards. Too much caution cannot be used in this respect, as I much fear the disease has been spread considerably by these means.

These remarks will, I trust, be received in the spirit in which they are given—viz., with a desire to do good. I do not mean to run down any system. I am ready to adopt that which is best. My idea is that the water treatment is the most simple, most rational, and most in accordance with nature. I may be wrong, but such is my experience. I believe in *nature* being the great restorer; and if the mechanism of the body gets out of order, if we can only assist its vital powers in the proper manner, it will easily conquer the disease. It won't do to use the internal organs as we would a cast-iron pipe, to cleanse and scour it as it were with a scrubbing brush: the organs of the system are too delicate, and must be treated with extreme caution, otherwise the cure may be worse than the disease. Water, therefore, properly applied, I believe to be the simplest and safest remedy that can be used to assist the restorative powers of nature.

Mr. Wm. Sim said—It occurs to me that Mr. Alex. Graham of Capellie, in a series of letters which appeared in the *Glasgow Herald*, advocated a system of wet packing very similar to that now stated in the paper. It was tried by him with but indifferent success—very indifferent indeed. I attach much greater value to the hint thrown out in the paper with regard to the use of disinfectants. We must all admit that rinderpest is a very mysterious disease, and that it is one which is conveyed by the atmosphere. We also know the great value of chloride of lime, used together with sulphuric acid, as a disinfectant in cases of cholera, and that it is an agent which can be very simply

applied. From a chemist in the city to-day I ascertained that 1 lb. of chloride of lime and 2 lbs. of sulphuric acid, which may be obtained for 7d., are quite sufficient to fumigate thoroughly the largest byre in the kingdom. I do believe that there has been a neglect in not making a more free use of disinfectants. Then, again, with regard to the interment of the carcass, as I understand the law as laid down by the Privy Council, it is this, that the dead animal shall be buried to a depth of not less than 3 feet from the surface of the earth, and quicklime of course put in with it. Now, I happened to be in the North of Scotland last week, and while waiting at a railway station I heard it stated in conversation that the hungry dogs of poachers, when out at night, actually scraped the earth away and exposed the carcass. So long as that state of matters exists, and gaseous products get into the atmosphere, there is certainly a danger that this may be carried, and cause the infection to spread. Now, I do not see why the process of incineration should not be adopted. It might be very simply done. All they would have to do is to kindle a fire of sticks, or anything else, hanging up the four quarters of animals upon a tripod of iron, and the fire would be self-acting. The product would be bare ash; and I should think there would be very little poison remaining, or any further danger to be apprehended.

MR. A. GRAHAM of Capellie said—The last speaker has referred to my letters in the *Glasgow Herald,* and said that my experiments met with indifferent success. I have only to say the success has been so indifferent that I have never made an experiment at all. I never saw an animal suffering from the rinderpest in my life; but I will say that, if I had such an animal, I could cure the patient in six, seven, eight, or, at most, nine hours; therefore, I have not failed. On the contrary, I have succeeded, because the disease has never entered my byres. I think the paper which has been read a very able treatise on this subject; but in two or three points, which are not material, I do not agree with the writer. In the first place, with regard to the packing, he puts on one wet sheet: my theory is altogether different. He also says he would not give medicine in combination; there again we disagree. If a cow showed symptoms of an inflammatory attack, I would proceed at once to give her a certain quantity of sulphur, of nitre, of ginger, and some treacle. This I would do for very obvious, or may I say in this Society, for philosophic purposes. The treacle, of course, is to affect the bowels, the sulphur to purify the blood, the nitre to affect the kidneys, and the ginger to give a tone to the stomach. I would then apply the packing, not putting on one sheet only, as Mr. Brown recommends. I would first wring one sheet out of cold water,

and put it carefully on the chest and round the neck, sewing it thoroughly, so as to exclude the air. Then I would put on a second and a third in the same way, because I prefer three separate woollen sheets to one sheet of the same thickness as the three. Then I would apply other three sheets. Now, here is an essential difference between Mr. Brown's idea and mine. My object is a good many folds. It is to produce a decided perspiration. I like to see it dripping from the beast: and I know when to remove the sheets, not by my watch, but by watching. Whenever I find that the three upper sheets are wet, and the under three dry, I pull them off, and put carefully over the animal a toasted rug or blanket. Mr. Brown said he would not give the medicine in combination with the packing: I say the virtue of the whole thing is giving them in combination. He says it will prevent the medicine from acting: I say it will make it act quicker and more surely; and quickness and certainty are a very great matter in such a case as this. I say further, my expectation would be, that in the course of a very short time, certainly within nine hours, you would find the animal chewing its cud. It may seem odd that I should speak thus, not having myself seen a case of rinderpest; but I have seen very many cases of pleuro-pneumonia; and I have no hesitation in stating my belief that at least as many cattle in this country have perished from pleuro-pneumonia as have yet died from rinderpest. It is to my utter surprise that I see, day after day, reports of deaths by rinderpest, yet that I never see one single report of the deaths by pleuro-pneumonia. Let me also express my surprise that, while our cattle are not permitted to be sent to Ireland, there is no prohibition against bringing Irish cattle to this country. It is true that no cases of rinderpest are reported in Ireland; but will any man tell me they have not had severe cases of pleuro-pneumonia, which is as infectious as rinderpest? But I wish to point out a difference between my mode of packing and its effects, and that which is advocated by Mr. Brown. He said it was the same as putting a poultice upon the animal: I tell you it is quite different from a poultice. If I wished to put a poultice, by a damp sheet, upon an animal, I would not put dry sheets, but a waterproof cloth on it to exclude the air, and that would produce something like a poultice. The gentleman who read the paper also spoke about the Turkish and vapour baths. My opinion is that both of these may be very good, and may be used very well. I do not wish to give any strong opinion upon this matter, because I have not seen it tried; but I have seen this system of packing most thoroughly tried in pleuro-pneumonia. I myself lost thirteen cattle by pleuro-pneumonia. In endeavouring to find out a remedy I tried medicines, and

so forth; and afterwards I tried this packing, since which time I have never lost an animal from pleuro-pneumonia. Having that experience, and being asked again and again to say something upon this subject, I wrote my first letter to the *Glasgow Herald*, in which I recommended a similar mode of treatment for animals suffering from rinderpest. I looked upon murrain, pleuro-pneumonia, and rinderpest as one and the same in their origin—namely, a disordered and inflamed state of the blood; and my opinion was, that if you could extract the inflammation, your patient was safe. But I have also a great idea of prevention as being better than cure. I believe that not one man in a hundred in our dairy district ever dreams of giving salt to his cow. That strikes me as utterly absurd. I have always been in the habit of directing salt to be sprinkled amongst the food of my cows, because it assists digestion, and for other reasons. I have now had cattle for at least fifteen or twenty years in my establishment, and I have never had a case of hoven, which I ascribe solely to the use of salt. Then, again, I have a very strong belief in regard to our dairy cows being kept in a most artificial state. They are fed, as country people say, "to the masthead," to make them produce any quantity of milk for the Glasgow market. Well, the consequence is, that their blood is, I think, in a most unnatural state. Therefore, what I do in my establishment is this:—Each milk cow gets a tea-spoonful of sulphur every day, and on the seventh day—the Sabbath, or Sunday, whichever you please to call it, but it is the seventh day at any rate—on that day the cow gets as much nitre as lies on a shilling, with a considerable quantity of ginger. You can easily guess the object of these things. I administer the nitre once a week, to keep the kidneys all right, while the ginger is given to tone the stomach. I have only further to say that in these remarks, and in anything I have written on this subject, my only object has been to promote the public good.

Dr. Lyon—I am a little surprised that the members of a Philosophical Society should listen to all the statements of Mr. Graham without some scientific-like proofs having been brought forward of the reasons upon which he acted. I am a little surprised that any gentleman not educated as a medical man should presume to give opinions upon this subject at all. It is a recondite subject—it is a very difficult subject indeed. Why, the best minds in all ages have been studying these matters, and we are as ignorant of them now as they were before. Mr. Graham, whose letters I have read, describes his treatment of the rinderpest by practice which, in his hands, he said, resulted in a cure, but which in the hands of every other person failed completely. In fact, the treatment was not based upon any scientific principle at all.

Mr. Graham is always looking to effects—he is forgetting the causes. He does not know what the cause is. Has he any influence over the cause? I say he has none. I say he is acting on the most ignorant ideas—upon perfectly empirical ideas. I beg you to understand I do not mean to be in the least degree personal; but in place of mere statements—vague statements, and unscientific-like statements—I think a philosophical society is entitled to have something of a very different description indeed. Why, what do we know about epidemic diseases of almost any kind? What are they? Where do they come from? How are they produced? No man is able to answer; and until we really know what the cause is, it is not very likely we will be able to counteract the disease. For Mr. Graham to suppose, for instance, that he could cure typhus fever or scarlet fever by the warm bath, or sweating, is out of the question altogether; and he has found that out; and all persons who have followed his recommendation have found it out too. The expectation that a little nitre, or sulphur, or salt, is to perform very important effects upon the economy of the animal is not founded on fact; and if you wish to discuss this subject in a scientific manner, you must proceed in a very different way from what you have been doing to-night. I was detained by my professional duties, and did not hear the paper read; but I think it is *ultra vires*—I think it is presumption—excuse me for saying so—for any man not educated as a medical man, and knowing the little control he has over disease, to speak in the way I have indicated. If he had the experience of a medical man he would come with much less dependence upon his opinions or his practice than Mr. Graham seems to have. But in making these remarks I desire you shall not take them in a personal manner; I am merely speaking my sentiments on the subject.

Mr. Graham—The gentleman who has just spoken is, I presume, a member of this Society. He is perhaps not aware that I am not here of myself; he is perhaps not aware that I am here by invitation; he is perhaps not aware that the words he heard me speak were not words that I volunteered, but that I was asked by you, sir, the chairman of this Society, to rise and say if I had any observations to make——

The Chairman—Certainly.

Mr. Graham—Therefore, sir, when a member of your Society speaks of the ignorance of a stranger, who is asked to display that ignorance in order that he might have it amended—when he speaks of it in the language in which I have heard him speak—am I to understand that that is the style of this Society altogether? I shall leave this room with an impression altogether different from that with which I entered

it—which I am sorry to do, because I see one or two gentlemen here for whom I have personally the greatest possible respect; but I will say that the language that has been used towards me is not language which I am accustomed to in common society, whether it be right or not in a Philosophical Society. And, sir, I want the gentleman who made these observations to remember that I never said I had treated a single animal labouring under rinderpest. I stated quite the contrary. He said I had failed, and that others had failed. I tell you I have never failed, because I have never tried; and I tell you that hosts and hosts of people, whose letters I have beside me, have succeeded well; and the only people who did not succeed, that I have hitherto known, have been self-sufficient people, who thought they knew better than I did, and do not follow exactly the process I prescribed, but took a process of their own, added to mine. As to the cause of this disease, I have said distinctly that it is simply a disordered or inflamed state of the blood. I went to the cause at once, and I have no hesitation in stating the cause.

The CHAIRMAN—In anything Dr. Lyon has said, I think Mr. Graham must admit that he meant to aim at great and broad principles, rather than to refer to anything that had been said here to-night. I think Dr. Lyon's remarks are correct with regard to the strictly philosophical view of the case; but in coming here to listen to the paper which we have heard, we came to learn what men thought and had been doing regarding this disease. If we are to discuss the subject philosophically, I think it becomes a different matter. I think Dr. Lyon was a little too severe in the way he took it up; and I am at the same time perfectly certain that he had no motive but to express his opinion that a Philosophical Society of such standing as ours ought to take a great and broad view of the subject. At the same time, I think we ought to feel deeply grateful to Mr. Graham for what he has given us. I beg to thank you, sir, for the very kindly way in which you have taken this, and I think what we have received from you can do no harm at any rate.

PROFESSOR ANDERSON—I am sure there can be but one feeling on the part of the Society with regard to the discussion which has taken place on the present occasion, and the thanks we owe to the gentlemen who have spoken and communicated to us their ideas upon the subject of rinderpest. There appears to me, however, to be one point in the whole discussion which comes very prominently forward, and it is this, that we have suggestions of the method of curing this disease which are comparatively unsupported by facts. We have not sufficient data, as it appears to me, to form any judgment as to the success of the pro-

posals which have been made. As I understand the matter, Mr. Brown's plan has not been tried; and Mr. Graham himself has not had an opportunity of employing the method which he has suggested. It would be most important if we could have a full statement of the results which have been obtained with regard to it. Certainly, judging from what has appeared in the public prints, I should infer that, at all events, Mr. Graham's plan is not always successful; and it would be most important if we could have some information of the causes of success in particular cases, and failure in others. Looking at what appears in the public prints generally, the conclusion to which I come is, that no plan whatever that has yet been proposed has been attended with success. I am one of those who are inclined to think that preventive measures are most necessary; and, like a previous speaker, I would urge the general use of disinfectants to a much greater extent than has hitherto been practised—not by a single fumigation, as that gentleman seemed to suggest, but by the continuous use of disinfectants, such as chloride of lime. I also believe that the utmost attention should be paid to cleanliness; in fact, that sanitary precautions should be adopted in the byres as much as possible. I am far from supposing that it would be possible to prevent rinderpest by such precautionary measures; but I am satisfied they would do a great deal to diminish the intensity of an epidemic when it made its appearance.

Mr. William M'Adam said he thought Mr. Graham should take some opportunity of testing his mode for the cure of rinderpest, so that it might be proved whether or not it was a correct remedy.

Dr. Lyon afterwards offered some remarks. He alluded to the mystery in which the origin of epidemics was felt by medical men to be shrouded, and went on to say that they knew it was perfectly futile to suppose that perspiration would carry off the disease now under consideration. In many such diseases perspiration was one of the most common symptoms, and yet the poison was not carried off by any manner of means. Therefore, if this subject was to be discussed, they must take it up as a whole, and not go upon the supposition that a little sulphur, or a little nitre, or a purgative, or anything of that kind, would overcome the disease. It was probably not altogether hopeless, however, that some day or other antidotes to, or preventatives of, this disease might be found. Quinine, in some manner of which they knew nothing, had the effect of interrupting the progress of ague, and arsenic, in the form of ague drops possessed the same remedial power; and it was to be hoped that as science advanced some antidote, or preventative, or cure might be found for other diseases which at present baffled the skill of the attendant.

Mr. Brown said he would have liked if Dr. Lyon had come forward with something more definite than his very general remarks. Professor Anderson desiderated proof that his system had really been effective in cases of rinderpest; and he might state that Mr. George Hills, M.D., of Dunoon, bore testimony to its complete efficacy in the case of a cow suffering under most decided symptoms of rinderpest. The animal was subjected to the hydropathic treatment, and in two days was out of danger. In other cases of rinderpest his system had been applied; but as he was not there himself to see it fully carried out, the parties applied it for a few hours and then stopped it, and of course if it was not continuously applied a cure could not be expected. Dr. Lyon had spoken about the causes of epidemic diseases, and where they came from; but it might have been more satisfactory if he had told them some means of curing fevers——

Dr. Lyon—No occasion for that when you have got a cure, it appears.

Mr. Brown—Well, you do not approve of it.

Dr. Lyon—Your plan is to find patients and put your system into execution. You surely do not act a very public-spirited part if you do not announce your remedy, and offer yourself as an attendant upon these animals, and thereby interrupt the disease by your certain method of treatment. I think you are almost bound to do so.

Mr. Brown—I have done so to a certain extent; but it is sometimes not very easy for a man in business to find time.

Dr. Lyon—Oh, make sacrifices for that.

Mr. Brown went on to refer to cases in which the water treatment had been attended with the greatest benefit in diseases of the human subject. He believed that, when rightly applied, it was the most effectual means of cure, and that ere many years had elapsed medical men would espouse it far more willingly than they did at present.

The Chairman—I think this discussion has to some extent taken a form which is not desirable in discussing a great question of this kind. I think, with other speakers, that this disease called rinderpest arises from a subtle poison, the source of which has not been traced. We know, however, that it is contagious, and we have also satisfactory evidence that preventatives or disinfectants, when systematically applied, are highly beneficial. I have not the slightest doubt that the system of killing and burying introduced by Dr. Gamgee, and thought at the time to be a very savage one, is the proper principle—that when that system has been carried out definitively in any district it has never yet gone beyond that district. These are facts which Dr. Lyon may not think philosophical, but they are borne out by proofs which I

cannot resist, and I hold we are quite entitled to tender a vote of thanks to Mr. Brown for the nice paper he has read us, and also a vote of thanks to Mr. Graham for the kind way he has spoken, and the information he has given us with regard to his own practical experience. I sincerely trust this will not be the end of the discussion in the Philosophical Society upon such an important subject as this, but that Dr. Lyon himself will bring forward a scientific paper, and give us the theory of disease, which is most important.

Dr. Lyon—I shall not have time to do that.

VII.—*On the Determination of the Difference of Longitude between the Observatories of Greenwich and Glasgow by Galvanic Signals.* By Robert Grant, M.A., LL.D., F.R.S., Professor of Astronomy in the University of Glasgow.

Read February 21, 1866.

Professor Grant commenced with a brief description of the various methods which had been devised for ascertaining the difference of longitude of any two places on the earth's surface previous to the application of the principles of electrical science to the solution of the problem. The earliest method was founded on the observation of lunar eclipses. The entrance of the moon into the earth's shadow is a phenomenon which occurs at the same absolute instant of time wherever it is seen, and consequently the difference between the local times of occurrence, as noted by two observers occupying different positions on the earth's surface, will indicate the difference of longitude between the places of observation. A serious defect inherent in this method consists in the circumstance that, as the earth's shadow is not bounded by a well-defined outline, it is impossible to fix the instant of the moon's entrance into it with such a degree of precision as the delicate nature of the question requires. Besides, an eclipse of the moon is a phenomenon of comparatively rare occurence.

The discovery of Jupiter's satellites by Galileo, in the beginning of the seventeenth century—one of the first fruits of the invention of the telescope—suggested a new and more perfect solution of the problem. The satellites, in the course of their respective revolutions around their primary, undergo very frequent eclipses, and the instant of the disappearance of a satellite in the shadow of the planet, or that of its

subsequent re-appearance, as determined with reference to local time at two distant places, supplies the means of ascertaining their difference of longitude. This method has done good service to geography; but it is generally speaking inapplicable to astronomy and the more refined purposes of geodesy, from the circumstance that the observed instant of the immersion of the satellite in the planet's shadow, or that of its subsequent emersion, varies to a certain extent with the aperture of the telescope employed in the observation of the phenomenon. A third method is founded on the rapid motion of the moon in her orbit. In virtue of the earth's diurnal rotation, the moon is transported with a uniform movement from the more eastern meridian to the more western of any two assigned places on the earth's surface; but during this interval she has travelled over a certain part of her monthly revolution, and therefore her orbital position, when she is on the meridian, will not be the same at both places. Hence, by ascertaining the orbital position of the moon when she is on the meridian at each of the two places, and comparing the results, we obtain a knowledge of the space which has been travelled over by her during the intermediate interval; and by means of this result, and the theory of the moon's motion, we readily determine the time occupied by her in passing from the one meridian to the other, which obviously leads to a knowledge of the difference of longitude between the two places. This method is characterized by several disadvantages, one of which consists in the fact that an error of one second of time committed in observing the instant of the passage of the moon over either meridian, would entail an error of thirty times that amount upon the resulting value of the longitude. Better methods have been devised by astronomers, founded upon observations of eclipses of the sun, and of occultations of the fixed stars by the moon; but such phenomena are of comparatively rare occurrence, the computations are intricate, and are complicated profoundly with the theory of the moon's motion.

A method for ascertaining the difference of longitude between two Observatories, which has been extensively used in the present century, consists in transporting, by means of chronometers, the local time as determined at one of the Observatories to the other Observatory, and in this manner comparing together the local times of the two Observatories, a process which manifestly gives their difference of longitude. Three celebrated determinations of longitude have been executed by the transportation of chronometers between two distant places. In 1844 the difference of longitude between Greenwich and Valentia, a small island on the west coast of Ireland, was determined by this method. Valentia is the most western position of the British Isles;

and as it lies nearly in the same latitude with Greenwich, it is favourable for the measurement of an arc of considerable extent upon the earth's surface perpendicular to the meridian. The object which Mr. Airy, the Astronomer-Royal, proposed, in determining the difference of longitude between Greenwich and Valentia, was to ascertain how nearly the result agreed with the difference of longitude assigned by the great triangulation of the British Isles, with the view of discovering whether any alteration was necessary in the elements of the earth's figure which had been assumed as the basis of the Ordnance Survey operations. The observed difference of longitude was determined by the transportation of thirty chronometers to and fro eight times between Greenwich and Valentia. The result agreed very closely with the computed difference of longitude, insomuch as to induce Mr. Airy to conclude "that no improvement could be made in the earth's figure, so far as relates to the circumference of a parallel, or to the measure of one second of space on the arc perpendicular to the meridian, and that, in latitude 51° 40′ the length of one second of space on such an arc measures 101·6499 feet. The difference of longitude between Greenwich and Valentia has also been recently determined by the transmission of galvanic signals, and the result presents a most satisfactory agreement with that obtained by the transportation of chronometers. In the same year of 1844 the difference of longitude between Greenwich and Pulkowa, the Imperial Observatory of Russia, was similarly determined by the transportation of chronometers. Altona, in Schleswig, was chosen as an intermediate station. Sixty-eight chronometers were carried sixteen times between Pulkowa and Altona, and forty-two chronometers crossed the German Ocean sixteen times in the transmission between Altona and Greenwich. The resulting longitude was valuable, not only in establishing the astronomical relation between the Observatories of Greenwich and Pulkowa, but also in connecting the great system of triangulation in the east of Europe with the British and French arcs of the meridian, and their respective triangulations. The third famous operation of this kind is the determination of the difference of longitude between the Observatories of Liverpool and Harvard College, Cambridge, U.S. In 1850 as many as 175 chronometers were transmitted in the Cunard steamers between the two Observatories. The operation was repeated in 1855, when fifty chronometers were transported three times. The resulting longitude forms the connecting link between Greenwich and all the other longitude determinations of the New World. It is doubtless entitled to a high degree of confidence; but when the Atlantic cable has been successfully laid, it will be interesting to ascertain how nearly it

approaches to the result furnished by the more improved method founded upon the application of electrical science.

The determination of the difference of longitude between two places, by the transmission of electric signals between them, was first practised in America, in the year 1849, in the operations connected with the United States Coast Survey. The method has since been employed in various forms, and the longitudes of numerous places, both in Europe and America, have been determined by its application. Towards the close of the year 1863 an electric wire was laid between this Observatory and the College, with the view of being used for the transmission of true time to the city of Glasgow. An unbroken metallic communication having been thus established between the Observatories of Greenwich and Glasgow, the determination of their difference of longitude, which had been so much desired, now presented itself as an object of practical realization. Professor Grant accordingly communicated on the subject with the Astronomer-Royal, who most kindly acceded to his views, and placed the resources of the Royal Observatory at his disposal for the efficient determination of the difference of longitude of the two Observatories. Shortly afterwards, upon a representation kindly made to them by their chief engineer, Mr. C. F. Varley, the Electric and International Telegraph Co. most courteously granted the use of a wire from Glasgow to London during the whole of the time which would be necessary for carrying the operations into effect. The Company also supplied the Observatory with the necessary batteries, a recording apparatus, and the valuable assistance of Mr. Lessels, their resident engineer.

The period of operations extended from 1865, April 28, to 1865, May 26. The method employed on the occasion is extremely simple. When a star passes each of the wires of the transit telescope of the more eastern of the two Observatories, the observer, by tapping a key with his finger, completes a galvanic circuit, and the instant of transit is recorded on the chronographic apparatus of the Observatory; but the galvanic current, instead of going to earth, is made to pass along the line-wire to the recording apparatus of the distant Observatory, upon which also the instant of transit is in the same way recorded. A process exactly similar is repeated when the star comes to the meridian of the more western Observatory. In this manner each signalling star supplies two pairs of recorded transits, from which are derived a value of the difference of longitude, and also a value of the time occupied by the electric fluid in its passage from the one Observatory to the other.

The observers at Greenwich were Mr. Dunkin, Mr. Criswick, and Mr. Carpenter. The observer at Glasgow was Mr. Plummer. The

personal equations of all these observers have been determined at Greenwich. The observations in the present instance are accordingly all reduced to those of Mr. Dunkin, the standard Greenwich observer.

The stars selected for signalling purposes were twenty-eight in number, and were arranged in four groups of seven stars each, in such a manner that when the last star of any of the groups had passed through the telescope of the more western Observatory, the first star of the following group was shortly afterwards to enter the telescope of the more eastern Observatory. By this arrangement the signalling at the one Observatory was prevented from interfering with the signalling at the other. Stars were signalled on every favourable night between April 28 and May 26. It frequently happened, however, that while stars were visible at one of the Observatories, they were wholly invisible at the other. As may be readily conjectured, the Glasgow Observatory was in general more unfavourably circumstanced in this respect than the Greenwich Observatory. In fact, it was found that while Greenwich signalled as many as 185 stars to Glasgow during the period over which the operations extended, Glasgow signalled only 117 stars to Greenwich. These two numbers may therefore be considered, so far, as an indication of the relative goodness of the climates of Greenwich and Glasgow. In order that the method employed might be carried into complete effect, it was necessary that the signalling stars should be observed at both Observatories on the same night. In consequence of the generally unfavourable state of the weather this was found to be impracticable, except on four nights—May 1, 2, 22, 25; and it is upon the results obtained on these four nights that the determination of the difference of longitude between the two Observatories is exclusively based.

A comparison of the time of transit of a star as observed at Greenwich, and recorded on the Greenwich chronograph, with the time of transit of the same star as observed at Glasgow, but recorded *also on the Greenwich chronograph*, furnishes a value of the difference of longitude of the two Observatories, but a value *too great* by the time which the electric fluid occupies in passing from Glasgow to Greenwich. On the other hand, a comparison of the time of transit of a star as observed at Glasgow, and recorded on the Glasgow chronograph, with the time of transit of the same star as observed at Greenwich, but recorded *also on the Glasgow chronograph*, supplies a value of the difference of longitude of the two Observatories, but a value *too small* by the time which the electric fluid occupies in passing from Glasgow to Greenwich. This will be easily understood when it is considered that the time of transit of a star at Glasgow exceeds the time of transit of the same star at the

more eastern Observatory of Greenwich, and that the excess of the one result over the other is *increased* in the case of the star being signalled from Glasgow to Greenwich by the time of passage of the electric fluid; whereas in the case of a star being signalled from Greenwich to Glasgow, it is the Greenwich time of transit—that is the *smaller* of the two—which is increased by the time of passage of the electric fluid, whence the excess of the one result over the other is necessarily made *less* by the same quantity. Hence the apparent longitude derived from the Greenwich registers of a signalling star ought to exceed the apparent longitude derived from the Glasgow registers of the same star by twice the time which the electric fluid occupies in passing between Greenwich and Glasgow. It is plain, from this circumstance, that half the sum of the apparent longitudes indicated by each signalling star will give a value of the true longitude, and that half their difference will give a value of the time occupied by the electric fluid in its passage from the one Observatory to the other.

The following results, extracted from the observations of May 2, are given by way of illustration. The minutes in the second, third, and fourth columns are omitted:—

STAR.	Apparent Longitude, derived from the Greenwich Registers.	Apparent Longitude, derived from the Glasgow Registers.	Concluded True Longitude.	Concluded Time of Passage of the Electric Fluid.
	Sec.	Sec.	Sec.	Sec.
Tau Virginis,..................	10·77	10·74	10·755	0·015
Lalande 25818,..............	10·62	10·56	10·590	0·030
Lalande 25892,..............	10·73	10·66	10·695	0·035
Lalande 25943,..............	10·63	10·51	10·570	0·060
d Bootis,........................	10·52	10·50	10·510	0·010
26 Bootis,......................	10·70	10·60	10·650	0·050
Sigma Bootis,................	10·69	10·61	10·650	0·040
Lalande 26896,..............	10·72	10·62	10·670	0·050
B. A. C. 4846,................	10·62	10·59	10·605	0·015

The following are the resulting values of the difference of longitude for the four days of signalling:—

May 1, . . .	17 m. 10·680 sec.	May 22, . . .	17 m. 10·433 sec.
— 2, . . .	10·648 „	— 25, . . .	10·463 „

Combining these results together, with a due regard to the number of signalling stars on each day, we obtain the longitude of the transit circle of the Glasgow Observatory,

17 m. 10·55 sec. W.

This result differs only a little more than a tenth of a second from any of the foregoing individual values. The value of the longitude in arc is

$$4^\circ\ 17'\ 38''\ 25\ \text{W}.$$

For the time occupied by the electric fluid in its passage from the one Observatory to the other the result obtained was—

0·029 sec., or 1·30th of a second nearly.

Assuming the distance between the two Observatories to be 400 miles, this would indicate a velocity of about 12,000 miles in a second.

PROFESSOR GRANT expressed his warm acknowledgments to the local officials of the Electric and International Telegraph Company, who were most obliging on all occasions, and rendered very effective aid in the conducting of the operations.

PROFESSOR WILLIAM THOMSON said he had listened with much interest to the account which Professor Grant had given of the recent important operations for determining the longitude of the Glasgow Observatory. There were several curious circumstances which suggested themselves in connection with these operations, one of which was this, that while the individual values of the longitude, as derived from the signalling stars on the same day, agreed in general within a few hundredths of a second, the mean values for the separate days presented a difference of as much as two-tenths of a second. He would like to know from Professor Grant how this was accounted for.

PROFESSOR GRANT, in reply, stated that the circumstance, beyond doubt, was attributable to minute outstanding errors in the adjustment of the instruments used at the two Observatories in the signalling of the stars. Such minute errors would not exercise any appreciable influence on the individual results *inter se* for any given day, the signalling stars being, for convenience in other respects, confined within a comparatively narrow range of north polar distance.

PROFESSOR WILLIAM THOMSON remarked that it would be expedient if operations of this nature were repeated from time to time, as it was probable that the longitude so determined might vary to a slight extent from the difference of local attraction caused by the extensive mining operations carried on in the country. He would like to know whether a comparison had been instituted between the longitude of the Glasgow Observatory, as deduced by Professor Grant in the present instance, and the corresponding result computed from the triangulations of the Ordnance Survey.

PROFESSOR GRANT stated, with reference to Professor Thomson's

last remark, that he had recently some conversation on the subject with Lieutenant T. P. White, of the Royal Engineers, the superintending officer of the Ordnance Survey in Glasgow. Mr. White was not aware of any geodesical determination of the position of the Glasgow Observatory having been effected in connection with the operations of the Survey; but he kindly stated that he would write to the Head Office in Southampton, and would in due course supply him with the required results, if any such had been executed. He hoped, therefore, on some future occasion to lay before the Society the result of the comparison alluded to by Professor Thomson, which could not fail to be interesting.

VIII.—*On the Function of Articulate Speech, and on its connection with the Mind and the Bodily Organs; illustrated by a reference to recent Observations on certain Diseased States of the Brain.* By W. T. GAIRDNER, M.D., Professor of Practice of Physic in the University of Glasgow.

Read March 7, 1866.

THE subject of this paper is one which I have been led to believe might be interesting to the Philosophical Society of Glasgow, inasmuch as it concerns a faculty that, in the most ancient records of the human race, has been singled out as specially characteristic of humanity; a faculty which undoubtedly is closely connected with all the most glorious attributes of humanity—with those functions of man wherein he most plainly differs from the rest of the animals, and most definitely reflects the divine image in which he was created. When, in the second chapter of Genesis, God is represented as bringing every beast of the field and every fowl of the air to Adam "to see what he would call them;" and when it is said that "whatsoever Adam called every living creature, that was the name thereof; and Adam gave names to all cattle, and to the fowl of the air, and to every beast of the field,"—there is surely something signified in this remote and venerable tradition beyond a mere act of arbitrary nomenclature. It is, if I mistake not, a witness to the antiquity of the belief that the devising of some form of articulate speech must have been one of the first acts of the essentially human free will; and further, that the first effort of speech beyond a mere inarticulate cry must have been the mental discrimination, and afterwards the designation, or *naming*, of objects by a kind

and quality of mental effort differing from that which is possible—even after ages of education by the side of man—to the dog, the horse, or the elephant, or even to our quadrumanous cousin (as some will have him) the ape, with his wonderful mimicry of the lower attributes of humanity.* So, too, when it is said afterwards in the same record, that "the whole earth was of one language, and of one speech," and that by means of this community of language the foundations were laid of that tower whereby the aspirations of humanity became apparent—whereby it became possible to hope that man might yet scale the heavens, and sit in the seats of the gods, so that God himself said, "The people is one, and they have all one language; and this they begin to do: and now nothing will be restrained from them, which they have imagined to do. Go to, let us go down, and there confound their language, that they may not understand one another's speech,"—it is impossible not to recognize in this poetic legend the impression, early implanted in the human race, that the faculty of speech is by far the greatest and proudest of human corporeal faculties; so great and so proud that when skilfully employed, it will sway men almost irresistibly for good or for evil, and become a power literally all but infinite, subduing almost everything, except omnipotence, to the will of man; ay, and at times even appearing, in the recklessness of its unscrupulous aspirations, to defy the power of God himself. But it is not my object at present to follow out these ideas, or to enter upon or suggest disputed questions of philology, or of ethnological science. I put these forward simply as preliminary considerations, tending to show the sources of interest which scientific men of all classes,—theologians, metaphysicians, physiologists, ethnologists, students of language of every kind—may find in researches having for their object the more difficult and obscure relations of the faculty of speech with the mental and bodily organs through which it is produced.

* "We must concede to animals (says Professor Max Müller) 'sensation, perception, memory, will, and judgment;' but we cannot allow to them a trace of what the Greek called *Lógos*—*i. e.*, reason, literally gathering, a word which most rightly and naturally expresses in Greek both speech and reason. *Lógos* is derived from *légein*, which, like the Latin *legere*, means, originally, to gather. . . . *Lógos*, used in the sense of reason, meant originally . . . nothing more nor less than the gathering up of the single by means of the general. . . . But *Lógos*, used in the sense of word, means likewise a gathering; for every word, or at least every name, is based on the same process: it represents the gathering of the single under the general. As we cannot tell or count quantities without numbers, so we cannot tell or recount things without words."—Max Müller, *Lectures on the Science of Language*, 2nd series, Lecture II., pp. 63, 64.

For the same reason as is suggested above,—viz., that I desire for the most part to adhere to facts within the reach of personal verification, and not to express opinions on difficult and doubtful questions of primary science or philosophy,—I refrain from discussing the mode of origin of language, or of languages, and particularly, the mode of outgrowth of complex expressions from more simple ones; a subject which has never ceased to have a great interest for me since I read the *Diversions of Purley* nearly a quarter of a century ago, but on which, nevertheless, I feel very little able to enlighten the members of this Society. It may, perhaps, be taken for granted, amid all the differences of opinion on this subject indicated by Max Müller and others, that *names* or *nouns* are, in a certain sense, the aboriginal nuclei of articulate speech, and that between the utterance of an inarticulate cry or interjection, such as might be used by any dumb animal in obedience to its primary instincts, and the measured and stately periods of a Demosthenes, there lies, as a first step, a certain special intellectual process peculiar to man, in virtue of which he alone of all animals is competent to overleap the gulf between the mere recognition of an object, and the designation of it through those bodily organs which the Creator has specially framed so as to give ample expression, in every stage of civilization or of barbarism, to the thoughts and emotions of "articulate-speaking men." *

What is the nature of this first and simplest process of human utterance? To what defects is it subject? With what mental attributes is it related? How does it stand related, for instance, to *memory, reflection, emotion, intellect, pure reason?* Can it be distinctively viewed as a separate faculty, and if so, how brought into connection with the other faculties? Can it be supposed to have a separate seat in the bodily organ of mind, the brain, and to be the subject of separate physiological and pathological derangements? What is, or what would be (if we could come to know it), the state of a man in whom all his other powers remained, but in whom we may suppose that not only words, but the very *idea* of words, had been totally and suddenly lost? Have we any experience of such a state? And if, after investigation, we feel bound to admit not only that such a condition is conceivable, but that, in various degrees, and with various modifications, it actually exists, on what principle should we view such mutilated specimens of humanity, I do not say in relation to their cure (for I do not wish to make this

* See *Note* at the end of the text of paper on the primary *Roots, or Nuclei, of Articulate Speech;* and particularly on the relation of the noun to the verb in primitive languages.

Society an arena for strictly medical questions), but in connection with questions of legal capacity, personal and political freedom, moral responsibility to God and to man?

Such are some of the questions that will be raised by the facts I have to state to you to-night, and which, I venture to think are well worthy of the attention of the Philosophical Society. I shall not, however, attempt to answer, or even to discuss in a thorough and satisfactory manner, most of these questions. My object is rather to furnish you with materials for their discussion, or at least to suggest them to the minds of thoughtful men in every department of scientific inquiry, by means of facts drawn from the realm of disease; of which, therefore, a physician may claim, without presumption, to be an instructor of men much wiser than himself, provided he keeps faithfully within the boundaries of his own carefully-watched experience, or the experience of others whom he knows to be trustworthy and skilful observers, and thus worthy of your attention.

In the present instance the class of facts I have in view is one which has formed the subject of a brilliant discussion in the Académie de Médécine in Paris, and which, besides having been casually noticed by numerous observers from an early period, has been most elaborately illustrated of late by MM. Auburtin and Broca,* and by M. Trousseau,† who has given to the condition above referred to the name of *Aphasia*—a name approved by the distinguished Greek scholar M. Littré, and which will probably displace the names of *Aphemia* and *Alalia*, applied by other observers. But for those who may be desirous of arriving by a comparatively short route at the medical conclusions of all these observers and others, stated in a remarkably accurate, thoughtful, and entirely trustworthy manner, though more technically than will be possible to-night, I can recommend with confidence a short paper in the *Edinburgh Medical Journal* for March, 1866, by my friend Dr. Sanders, who has been kind enough to favour us with his company at this meeting, and may probably be able to contribute something to the discussion of this paper.

Now and then in our hospitals, now and then in private practice, and occasionally, but (so far as I have had opportunities of learning) rather rarely in our asylums, cases occur of which the following may be regarded as a generalized description. The persons referred to have been the subject of symptoms of some kind referred to the brain, usually sudden, sometimes with, but sometimes also without, a

* Société d'Anthropologie de Paris, 1861, in *Gazette Hebdomadaire.*

† *Clinique Médicale de l'Hotel-Dieu,* Tome 2 (2d edition), ch. 58.

manifest disturbance of the intellect. Sometimes there has been an epileptic attack, sometimes a disorder more resembling apoplexy; in the latter case there is often paralysis, and the palsy is then (curiously enough) almost invariably upon the right side of the body, and upon it only. The palsy may be of every degree of completeness; the patient may be confined to bed, or not; he may be unable to walk at all, or may walk with difficulty, or only with a very slight halt; he may be unable to move his right arm, or to hold a pen, or there may be scarcely any perceptible diminution of power; he may be more or less altered in the expression of his face, from palsy of one side of the mouth; in many cases, however, he has complete command over the movements of the tongue and lips, at least to such an extent as to show that it is not from paralysis that he is unable to carry on a conversation. The states of the intellect and consciousness are equally variable: sometimes there is greatly diminished sensibility to external impressions, with apathy and listlessness, or inattention to the demands of nature; at other times there is the most acute sensibility, with apparently strong desire on the part of the patient to communicate the emotions which affect him; he will nod, wink, laugh, weep, look pleased or angry, give every evidence of strong affection towards those he loves, and be not at all sparing in demonstrations of dislike to those with whom he is offended; he will even be very obstinate, insist on what he thinks to be his rights, take his place at table, will carve, pour out and drink wine, and generally do the honours as a host, sometimes with a certain awkwardness, but in other cases with grace and courtesy; he will dress and undress himself, will come and go at regular hours, and behave in most respects like other persons living in a family; he will play such games as backgammon and croquet, or even whist, without any very important errors, and in some cases as well as ever in his life; he will observe anything that is out of its place, and will rectify it; and so far from being a passive or apathetic being, he will often insist on having his own way, and will show great vexation if his desires are interfered with. In one case, for example, of this kind which I had special opportunities for observing, the patient could hardly be restrained from behaving as if he was in the most absolutely sound bodily and mental health. Having been attacked about the 9th of August, and having been for some days in a state of partial insensibility, his very first thought, apparently, on emerging from this, was of his gun; and for many days after he had so far recovered as to be able to leave his room, he busied himself in searching everywhere for powder and shot, which had been carefully put out of his reach; showing, moreover, in a way almost alarming to his relatives, his strong displeasure at being debarred from his coveted sport; yet

this gentleman, at a subsequent stage of his disorder, finding a gun in the house belonging to a visitor, and loaded, was so far guided by his habitual sense of order and propriety as to interfere immediately, and very judiciously, with what he considered to be an unsafe arrangement, to remove the percussion caps carefully, and lay both them and the gun aside in what he thought safe positions, where they were found with some difficulty by their proper owner. In short, although from circumstances presently to be stated there is a great and almost insuperable difficulty in arriving at a just estimate of the mental condition of patients in what I am about to describe as the *Aphasic state*, it is evident in many of these cases that the mind is at least so far unaffected as to permit of the exercise of all its ordinary active functions to a certain extent, and there is no direct proof of even seriously impaired intelligence; while it is equally plain that all the ordinary emotions and voluntary acts, with the single exception of those implicated in the expression of the thoughts in words, are performed without much difficulty, and often in a perfectly normal manner.

But as regards this *function of articulate speech*,—or rather as regards every faculty that centres round a *word*, or a *name*, as the representative of a thought,—there seems to be, in these cases, some mysterious barrier interposed between the thought and its expression; and it is rather remarkable that it is always the primeval faculty of *naming objects* (or of distinctively designating ideas present to the mind as objects of thought), rather than the more conventional forms of ordinary speech, that is most apt to suffer in these cases. In the more trivial cases of aphasia, indeed, there is little else, apparently, than an aggravation of a very common defect—the liability to forget, or to be unable to find the name of a thing, or of a person, when wanted. Thus, I have known an aphasic who could conduct an ordinary conversation pretty intelligibly, but who never could, even after repeated trials, name the days of the week, or even any two of them, in succession. He would say of Monday, that it was "the first working-day," and of Saturday, that it was "the day before the Sabbath." He would say of his aunt, that she was his "nearest relation by the mother's side," and would repeat this again and again. The same man had completely fulfilled what we are accustomed to hear ordinary and every-day people speak of as the acmé of forgetfulness, when they cannot find a word—he had in fact forgotten, or at least could not give utterance to, his own name, and never could be made to pronounce more than one-half of it during the time I had him under observation. He was, however, perfectly aware of his own defect, and was even amused at it; nor would it have been easy to persuade him that the defect of utterance

was anything more than a slight infirmity of memory, as he was to all appearance quite intelligent, and able to communicate what he had to say, for the most part, with only slight and occasional difficulty; which could be got over, and was usually in fact got over, by some process of circumlocution. When first seen, indeed, this man talked so much unintelligible gibberish in the night that the nurse of the infirmary ward thought he was evidently delirious; but he had no difficulty in explaining to me next day that this was not at all the case; that he was perfectly aware of his own condition, and was apparently passing over from a state of perfect aphasia into the modified condition above described; that he was, in fact, practising speech, and that deliberately; that he felt he was "getting better and better every hour," and that he had "now got a better way." The earnestness with which this patient repudiated the imputation of delirium was most remarkable; and the very clear way in which he managed to convey his own impressions of his case, even while his tongue was so much paralyzed that he had to lay hold of it with his fingers in order to draw it out at my request, was a complete practical refutation of the idea that mental incapacity, in any ordinary sense of the term, was the source of his defect of utterance. This man, like the majority of aphasics, had suffered an attack of paralysis on the right side.*

While this paper is passing through the press, I have been enabled, through the kindness of Dr. Weir, to see another most instructive case of aphasia with paralysis of the right side, which has some interesting points of relation, and also of contrast, with the foregoing. In this case the patient is a lady, most carefully and intelligently tended by her husband and by a daughter, who have devoted themselves to the systematic education of the dormant faculty for the last two years; a consideration of no small importance in the estimate of what follows, and one which gives to cases of this kind, observed in the privacy of the home, and among affectionate relatives, a far higher value in general than can be attached to cases in hospital practice. This lady suffered, about two years ago, an attack of nearly complete insensibility, lasting for two or three days, and then passing gradually off, but followed at an interval of some days by paralysis of the right side, after which she

* A patient, whose case has been communicated to me, who is, like this man, paralyzed on right side, and acutely sensible of his own condition (having been a man of cultivated tastes and much addicted to reading), deplores very frequently and pathetically his inability to follow his usual pursuits; and in conversation, when he misses a word, is in the habit of saying, with expressive gestures,—"Ah! it's gone! gone into the waste-paper basket!"

became quite suddenly speechless, and also unable to protrude the tongue. She has since been subject to occasional, and not very severe, epileptiform attacks, but in the intervals has always preserved a considerable degree of intelligence, and since her partial convalescence has even entered with a renewed interest into the details of the family life. She recovered the use of her limbs to a considerable extent, and also all the ordinary movements of the tongue; she became able, also, to mutter a very few words now and then, and to follow with intelligence and interest the course of ordinary conversation, or of reading, within certain limits; but even now her attempts at conversation, although guided apparently by a wonderfully clear intelligence, and by the most unquestionable evidence of a keen and sustained interest in what is said to her, for the most part end in quite inarticulate sounds, or at most two or three brief formulæ of words. In this case, as in the preceding, there may, no doubt, have been at first a mixed condition of aphasia proper with paralysis of the organs of speech. But as regards the state of the mental capacity, I cannot better define the state of my own impressions than by means of the following dialogue, which I give nearly as it occurred; if, indeed, that may be called a dialogue which was conducted on one side almost entirely by signs, and by inarticulate sounds aided by signs. Taking her hand after a lengthened investigation, in which she listened with keen interest, and even with eagerness, to everything that was said on her behalf by her husband and daughter, I said,—'Madam, pray attend to me now very closely, for I wish to be sure that you understand thoroughly (a movement of intelligent sympathy). Are you sure that you understand? (a quick and eager nod.) Your right hand is weak, but you can knit quite well with your left? (affirmative nod.) Could you not *write* with your left hand? (sign of doubt.) I knew a lady much older than you, who learned in a very short time to write with her left hand when her right was disabled—Will you try? (affirmative nod.) You must take lessons every day, and see what you can do (many and most emphatic nods). Now, there is another thing. Tell me this, and be sure you follow me. Do you understand *every word* I am saying? (affirmative nod.) Quite well? (the same.) Now, observe; when you first began to recover from the palsy was it so? (oh, no!) You did not understand then? (negative sign.) Did you not understand a word? (the same.) You have learned, then, gradually to follow what is said to you? (affirmative sign.) And you now have no difficulty in understanding what at first you could not follow? (the same.) Well then, I will tell you what is your condition at present—listen. You have got back the idea of the words; you know quite well what you are

thinking, and what you *mean* to speak, but you have lost hold of the machinery—is it not so? (emphatic assent.) The machinery is out of gear—it has been long out of use—it wants to be practised and used—you must go to school again and learn; but you have got over the great difficulty—the words are there, in your head (a quite clearly intelligent assent). Set to work then, immediately, and try what you can do both in writing and speaking. You will come to it in time.' Throughout all this conversation it is impossible to convey on paper a sense of the clear, intelligent apprehension of every separate phrase which the patient managed to convey by means of action; but it was felt both by Dr. Weir and myself to be consistent only with the idea of a state of mind, if not absolutely unimpaired, at least as little impaired as it is possible to conceive in so shattered a state of the nervous system; and certainly in no way differing from the state of mind of many of those hemiplegic patients who have the most complete command of the faculty of speech.*

And here it seems necessary to establish clearly a distinction, which is of the first importance as regards what remains to be said. Aphasia of the kind here indicated, and probably all true aphasia, in the scientific sense of the word, is something entirely different from mere paralysis of the organs of vocal expression, even when those organs are, or may be supposed to be, paralyzed. And the proof of this is twofold. For *first*, Aphasia may be so complete as to leave only a very few words capable of being spoken, and yet those few words may be pronounced so as to show that there is no paralysis. But, *secondly*, it is quite well ascertained by medical observation that no amount of paralysis will cause the degree of obstruction to the utterance, even of single words, that we observe in aphasia. The bodily organs of articulation are so multiform, and under the dominion of so extended a range both of nerves and muscles, that it seems to be quite impossible to produce *speechlessness*, in the sense of entire unintelligibility of utterance, by almost any amount of damage to these organs consistent with life. Thus voicelessness, or *aphonia*, is quite common, but leaves the speech perfectly intelligible. Paralysis of the lips may proceed to the most absolute immobility, so that the patient laughs a hollow laugh behind a face as

* In a letter from this lady's husband, written after the above had been submitted to him for perusal and correction, a very interesting addition is incidentally made to the phenomena of Aphasia in this instance. "I forgot to notice (he writes) a remarkable thing in Mrs. ———'s case; she had a very fine ear for music, and now, though she cannot utter the words, she joins in our family worship, and follows the time as correctly as ever she did, taking every note, and sounding it sufficiently loud to enable us to hear the melody from her."

fixed and expressionless as a turnip lantern, and yet there will be no difficulty whatever in giving complete expression to the thoughts. In the extremest forms of hemiplegic paralysis, in which the tongue and face, severally or together, or the tongue, palate, and larynx simultaneously are affected; in the thick mumbling articulation of general paralysis of the insane; in the hurried and at the same time hesitating utterance of mercurial palsy, or of ordinary stammering, it is always possible to understand what is spoken, with a little more than ordinary attention. The extremely rare condition of complete paralysis of the tongue, *on both sides*,* comes perhaps nearest to the ideal of paralytic speechlessness that might be confounded with aphasia; but even here it is probable that there would be a possibility of vocalization, in most cases sufficient to give some expression to the thoughts; and at all events the rarity of these cases is such that they can scarcely affect our inferences. Nay, Mr. Syme has had, of late, a very curiously complete opportunity of pursuing the inquiry a step further, and of showing, in the case of a patient who had recovered from the very formidable operation of removal of the entire tongue for cancerous disease, that the excision of this "little member" which "no man can tame," even down to the hyoid bone, will not do much towards ridding the world of that "unruly evil" contemplated in the apostolic denunciation. In other words, it is made as plain as a fact can be made by an extreme instance, or *instantia crucis*, that intelligible speech is possible, nay, easy and wonderfully little altered, even in the absence of a tongue. So that complete and absolute inability to utter a single sentence, and sometimes even a word, as the spontaneous expression of deliberate thought, combined with the usually perfect articulation of the few words which can be spoken, or of the almost instinctive utterances which often arise under the influence of emotion, amount, when taken together in characteristic instances of the disease, to the most absolute demonstration that the aphasic state is in no way allied to paralysis of the muscular parts concerned in the mechanism of articulation.

Now this peculiar infirmity, as I have said, may exist in every degree, from the occasional and temporary difficulty of finding a particular word, to the absolute suppression of every kind of articulate verbal expression, and also of every kind of voluntary associated movement founded on the mere idea of a word. There are also differences in the mode of manifestation of this peculiar disease which are well worthy of attention. In one well-marked case of aphasia without paralysis, or at

* See one case of this, recorded by Dr. Hughlings Jackson, in *London Hospital Reports*, vol. i., p. 368.

least absolutely devoid of all trace of paralyzed articulation, to which I have already made reference in this description, the patient, though carefully watched, and living in the constant society of affectionate relatives, was not known to have spontaneously named more than one person during his whole illness, and her only by the first articulate word of infancy, "Mamma." Yet this young man could very easily and very perfectly articulate such expressions as these,—"I want—I want—I want . . . ,—Where's the . . . ," almost always stopping short at the name of the object, but on one or two occasions, at least, succeeding to the extent of repeating what he was in search of, after some one else had used the appropriate word. On another occasion he puzzled for awhile over the name of a new acquaintance, and finally gave in to the suggestion of "Mr. Thingumbob," which he pronounced several times over without any apparent difficulty. Frequently he would say—"Oh, I don't care," or—"Oh, it's no matter," or—"I don't believe," as a means of closing an attempt at conversation; and at last he became rather ingenious in conducting himself so as to appear attentive to conversation, and would often throw in a remark or two of the kind above noted, which would leave on a stranger the impression that he had carefully followed what was said. What makes very striking the absolute inability to apply, and perhaps even to understand when applied, the distinctive names of his most familiar friends in this case, is that this gentleman had no difficulty whatever in following out all the personal suggestions that occurred in family conversation, whether indicated by signs, or by photographs, or by the actual presence of the persons named. But when I one day named to him his brother, after the following fashion—"Your brother John—Jack, you know—John—Jack—has been ill, poor fellow,"—it was quite clear to me he did not apprehend to whom the remark applied. I had taken pains, however, in so speaking, to make no suggestion, by signs or otherwise, as to the person indicated, and a moment afterwards, giving a hint by a gesture, it was followed up immediately, and responded to by a look of clear intelligence. On another occasion his mother gave him a sum of money, which he distinctly understood to be wages intended for one of the servants. He was very anxious to undertake the commission, and seemed perfectly to understand that he was to be the bearer of the money. "Now, pay that to —— ——" (naming a very old servant, with whom he had been acquainted since childhood); "the gardener, you know, —— ——" (repeated several times). He went off on the errand immediately, and gave the money, without a moment's hesitation,—to the coachman! There was something very singular in this combina-

tion of complete intelligence as to the essential character of this mission, which he could easily infer from the fact of the sum of money itself, with complete failure as to the *person*, to which he could only be guided by words; and this, I think, is extremely characteristic of a state of highly developed aphasia. Sometimes, however, words which cannot be spoken spontaneously are clearly understood when spoken by others; at other times this seems not to be the case, and a word which in the normal state would be a familiar one, will be caught up and repeated mechanically, without any trace or shade of meaning being attached to it. Sometimes an aphasic will have a vocabulary absolutely limited to one or two words, either simple and common ones, as "Yes," or "No," or "Oh yes!" or mere interjections and exclamations, or perhaps utterly unintelligible combinations, as in the case of one of M. Trousseau's patients, who for four months had responded nothing but "*cousisi*" to every kind of address, and under all possible circumstances. "Whether he is in a passion or showing his gratitude, whether he is asking for a thing or refusing it," we are told in reference to this patient, "the 'cousisi' invariably follows, over and over again. Nevertheless, in moments of great irritation he articulates, 'Sacon, Sacon,' probably an abbreviation of the oath, 'Sacré nom de Dieu!'"

In regard to the use of oaths and other interjectional phrases, the facts are very curious. It is certain that aphasic persons who can articulate absolutely nothing else whatever, can sometimes give vent to an oath, or even several oaths, and no doubt they are sometimes greatly misunderstood in consequence of this peculiarity. I have always had a somewhat painful and regretful remembrance of a poor fellow who sought my aid in the Edinburgh Royal Infirmary many years ago, before the subject of this paper had attracted nearly so much attention as it has of late. He had fallen down on the street, or had been found in a speechless condition, and was brought in by the police. I found him free from fever, from all appreciable pain, and from paralysis, but he could scarcely be brought to speak even a single word, and all communication was purely by signs. At last, in visiting the ward at an unusual hour one day, I found he was being irritated and bantered by the other patients, who seemed to have much the same notion of him that village boys have of the town "natural" (idiot), —viz., that he was a legitimate butt for all kinds of coarse satire, and perhaps that he was only half a real invalid. I called one of the men to me whom I knew well, and asked if he believed —— was shamming? "'Deed, sir, it's very like it," was the reply. "How do you make that out? I'm sure he can't speak to tell you?"—"Na, sir, but

he can *swear* whiles. I think, sir, a guid whuppin' wad be the cure o' him."—"Very well, friend, you may think what you please, but you must not allow the men to bother him. I don't think he is shamming; mind that." I was not at this time fully aware of all the peculiarities of the aphasic in respect of swearing, but the result amply justified the poor fellow's good faith in applying for medical relief. He left the hospital very shortly afterwards, persecuted out of it, I am afraid, by his comrades in the ward, and was brought back in a state of insensibility, which ended in death in a few hours; his brain being found to be the seat of a large number of minute deposits of cancer.

One of M. Trousseau's patients, who had the remarkable and very exceptional peculiarity of paralysis on the *left* side of the body, had only two expressions when first seen—viz., "Ma foi!" and, on being provoked to speak, the mutilated oath, "Cré nom d'un coeur!" With these two expressions he answered all inquiries, and neither his name, nor his residence, nor anything whatever about him, could be discovered. He was accordingly dismissed from the hospital, but traced home by some of the officials, who succeeded in getting a history of him from some of his fellow-workmen. The attack was recent. The peculiarity of this case was, that while only the two expressions above mentioned were spontaneously used, and while no effort could procure either the name or the residence at first, the following conversation followed on his return to the hospital. "Are you not from Haute-Loire?" to which he replied like an echo—"Haute-Loire." "What is your name?"—"Haute-Loire." "What is your profession?"—"Haute-Loire." "But your name is Marcou?"—"Yes, sir." "You are sure your name is Marcou?"—"Yes." "What is your country?"—"Marcou." "Not at all; that is your name?" And then, with a gesture of impatience,—"Cré nom d'un coeur!"

It seemed afterwards that this man was quite conscious of the misapplication of his words, and in many cases aphasic patients are acutely sensible of their own blunders, although quite powerless to rectify them. Thus, a man who said "oui" to everything addressed to him indifferently, acquired the habit of marking by a significant gesture when he really meant "yes," and when, on the contrary, he meant to say "no." But "non" he could not say; nothing but "Oui—oui—oui." Marcou, on the other hand, was perfectly powerless to originate a single word other than the last one suggested to him by its sound; but when objects were named to him, was quite capable of indicating when the right name was given, and when the wrong one.

It is not always the case, however, that the sense of the true meaning of words perverted in the use is even thus far preserved. A

lady, whose case is also mentioned by M. Trousseau, and who gave no other sign of impropriety or of imbecility of conduct, who was also the mother-in-law of a physician, accosted a visitor as follows:—she rose up with an air of kindness to receive him, and pointing to an arm-chair, said "Cochon, animal, fichue bête!" which her son-in-law, standing by, and fully cognizant of her wishes, translated thus—"Madame vous invite a vous asseoir." She was apparently quite unconscious of the insulting expressions she had used, or rather supposed them to be the most ordinary courtesies.*

Possibly there was a similar unconsciousness in a case lately recorded in the *Gazette des Hospitaux*, in which an aphasic patient had only one word, which happened to be one of the most foul and loathsome obscenities of which the French language is capable. With this explosive compound she went about everywhere, apparently very little sensible of the dismay and astonishment she was creating.

There are many cases on record in which expressions more or less of the interjectional and explosive kind have emanated suddenly from aphasic patients by accident, and by a kind of surprise, as it were; the peculiarity of such utterances being that it was impossible to reproduce the same expressions by a voluntary effort. Such words as "Oh!" "Oh, dear!" "Dear me!" "A-deary-day!" "God bless my life!" &c., or the once fashionable and universal English oath implying the reverse of a blessing, but really, as it was habitually used, meaning literally nothing at all; or the corresponding, but possibly a shade more refined, French blasphemies, especially the unfailing and most irrationally abused "Sacré nom de Dieu!" have been in some instances almost the only utterances of aphasic patients beyond one or two of the simplest possible words; and they have seemed to be rather elicited by emotion, guided perhaps by old habits, than produced by anything resembling voluntary effort.

Of a similar kind is the expression "Merci!" which fell from a patient of M. Trousseau's on one occasion only—viz., when he had let fall his handkerchief, and a lady had picked it up and returned it to him. It seemed as though this word had been laid away from old habit, in some far-off corner of the mental organization, where it reposed beyond the reach of the volition, but ready for use when called up by the usual association of ideas. At all events it was found in this man

* A gentleman of unquestionable culture and good breeding, whose case has been incidentally referred to above, frequently salutes his friends with the most perfect courtesy of manner, but using the expression "brute," instead of the ordinary language of good society. He, however, is quite conscious of the impropriety, the objectionable word slipping from him as if *per incuriam*.

to be impossible afterwards, by any act of volition, to reproduce this word, his whole vocabulary being the single monosyllable "Oui"—and this, too, used somewhat interjectionally; as he could not put it together slowly, although he could point out the letters *o*, *u*, *i*, separately when named to him by another. So, too, the patient who so constantly repeated "Cousisi," could not be made, by any voluntary effort, to say either *coucou* or *sisi*, his parrot-like process being absolutely limited in its range to these precise syllables *cousisi* in their precise order, like the tune of the barrel organ after the barrel has been placed in the groove and the movement has begun.

Dr. Hughlings Jackson, who has observed numerous cases of aphasic disease with the care and disciplined habits of an accurate clinical observer, as well as the scientific instincts of a physiologist, has some excellent remarks upon this subject of interjectional, or, as I have called it, *explosive* speech, which I think the Society will the more readily excuse me for reading, since they are as brief as they are thoughtful and apposite.

"Swearing is, strictly speaking, not a part of language. It is a habit which has grown up from the impulse to add the force of passing emotions to the expression of ideas. It belongs to the same general category as loudness of tone and violence of gesticulation. The distinction of these from language as an intellectual act may be best illustrated by the remark Dr. Johnson once made to a boisterous antagonist, 'Sir, you raise your voice where you should enforce your argument.' Although oaths differ from mere alterations of tone, in that they consist of *articulate words*, they are generally used in talking, not to express ideas, but to make up by vigour in delivery what is wanting in precision of expression. They may indeed be considered as phrases which emotion has filched from the intellect, to express itself in more definite terms than it could do by mere violence of tone or manner. For oaths had once an intellectual meaning; they expressed ideas, and were uttered with a definite intention. Curses have in fact formed an element in religious services; but now-a-days intentional cursing is obsolete; it has degenerated to meaningless swearing, which, like cadence and gesticulation, is but a kind of 'commentary of the emotions [passions?] on the propositions of the intellect.' Vulgar people insert an oath 'at the proper intervals of their speech' as a sort of detonating comma, and thus they render forcible, statements which might otherwise strike their hearers as commonplace."*

It may be questioned, I think, whether this principle will not extend even a step further than is here proposed. I have already remarked

* *London Hospital Reports*, vol. i., p. 453.

that in all aphasic patients, so far as I have observed, names, and especially the names of persons and things, are the parts of speech which seem to be most effectually barred out from the possibility of utterance; and, indeed, a patient who has quite a little collection of stock phrases, including a good deal of what *appears* much more difficult and complex in idea than the naming of an object, will often be utterly unable to tell his own name, or that of any of his acquaintance, or to ask for the most common object in domestic use. I do not wish to prolong this dissertation, or I could give many instances of such phrases as "quite absurd—quite ridiculous,"—"I don't believe—I don't care,"—"I won't . . . "—"It's no matter,"—phrases the grammatical construction of which is certainly much more complex than that of a simple noun-substantive, and which were nevertheless freely employed by aphasics to whom a noun-substantive, standing alone and as a separate object before the volition, was for the most part as absolute a hindrance as a five-barred gate to a cart-horse. And what makes the matter more curious is that the noun-substantive could sometimes be surmounted with comparative ease; could be occasionally brought in by the way, as it were, in the midst of other vocables, when it would have been impossible, I believe, for these patients to have pronounced any such word taken alone and spontaneously; as in the phrase "It's no *matter*," which I take to have been used as strictly automatically and without any true consciousness of the applied meaning of its individual parts, as was the "*Cré nom d'un coeur*" in the mouth of M. Trousseau's patient, or the too well known English expression of comparatively *mild* damnation (quoted by Dr. H. Jackson in one of his cases) by which a man's *eyes* (in particular) are supposed to be devoted to destruction, in a formula which, verbally construed, has absolutely no conceivable meaning, and therefore could not possibly be used, even by the coarsest and most profane swearer, except as a mere expletive.

For the following interesting narrative bearing on this subject I am indebted to my colleague, Professor Pagan, to whom the case occurred in the Royal Infirmary about seventeen years ago:—"A youth, about sixteen years of age, was admitted, under my care, to the surgical ward of the Infirmary, with well-marked symptoms of concussion of the brain. About two hours before admission he had fallen upon his head, on the deck of a vessel at the Broomielaw, from a considerable height, having missed his footing on the rigging. He remained in a state of complete insensibility for nearly forty-eight hours after admission, when he began to speak in a language which could not be interpreted, till he was visited by one of his shipmates, who told us that it was Welsh. (It was a Welsh vessel, and the patient was a native of Wales.) He talked

incessantly, and we were told that his words were not incoherent, but rather a narrative of past events—recollections, in short. He continued in this state for between two and three days, when he began to speak most unmistakable English; but every word nearly was an oath, and most horrible imprecations he uttered. During three or four days longer he may be said to have *spoken* Welsh and *sworn* English. As consciousness gradually returned, he ceased to speak Welsh and to swear English; and when he recovered completely, after an illness of about twenty days, he spoke English and ceased to swear. There was no fracture of the skull, and but little external injury."

The explanation that seems to me, amid the confessedly great difficulties of the subject, to come as near as any to a satisfactory one of these and other apparent eccentricities of utterance in the aphasic, is that the words which are most readily spoken are always those which are prompted, not by external observation and deliberate volition, but by some internal association of ideas; the words being not so much deliberately spoken as set free (so to speak) by the fluctuating waves of emotion or of memory, which may be conceived to act upon the old established habits and latent capacities of the brain, very much as one wholly unskilled in music, or deprived for a time of his musical faculty, might let loose the tones of a barrel organ, without any direct personal cognizance of the music he is playing, or the precise combinations of movement he is calling into action. In other words, the aphasic is able to pronounce those words, and those only, that are *called out of him*, as it were, by trains of association of which he is only partially conscious at the time, acting upon forms and modes of expression which he may have learned to blurt out in the presence of similar associations long before his disease, while on the other hand he is utterly unable to select deliberately an appropriate term from among many inappropriate, and to apply it with free rational choice, and a correspondingly determinate effort of volition, to the expression of a predetermined idea then and there accurately defined in the consciousness. And hence he stumbles most of all at names, which are at once the simplest, the most primeval, and the most determinate of all utterances, while he is comparatively expert at phrases, which are the mechanical slaves of habit, the vague expressions of the inner consciousness or of old associations; and, in many cases at least, he is quite at his ease and fluent in oaths, which are the senseless and often nearly meaningless expletives derived from a past vocabulary, and exploded, amid his verbal difficulties, with as little consciousness of mischief as when a somnambulist in the midst of deep sleep sets fire to a barrel of gunpowder. Without

being quite assured that all the peculiarities of the aphasic will submit to be tested by this criterion, I think it helps to explain the most obvious, and at the same time some of the most surprising facts of his condition; *e. g.*, his absolute inability to pronounce some phrases *voluntarily*, while the same phrases are readily and even profusely poured out under mental excitement; the utterance of phrases *en masse* which he is incompetent to utter in detail; the confusion, misplacement, and often blank incapacity of utterance which specially bars the application of a noun or proper name to its object, even when that very noun or proper name can sometimes be elicited improperly by some roundabout process, in the midst of a phrase not so accurately determined by voluntary effort.

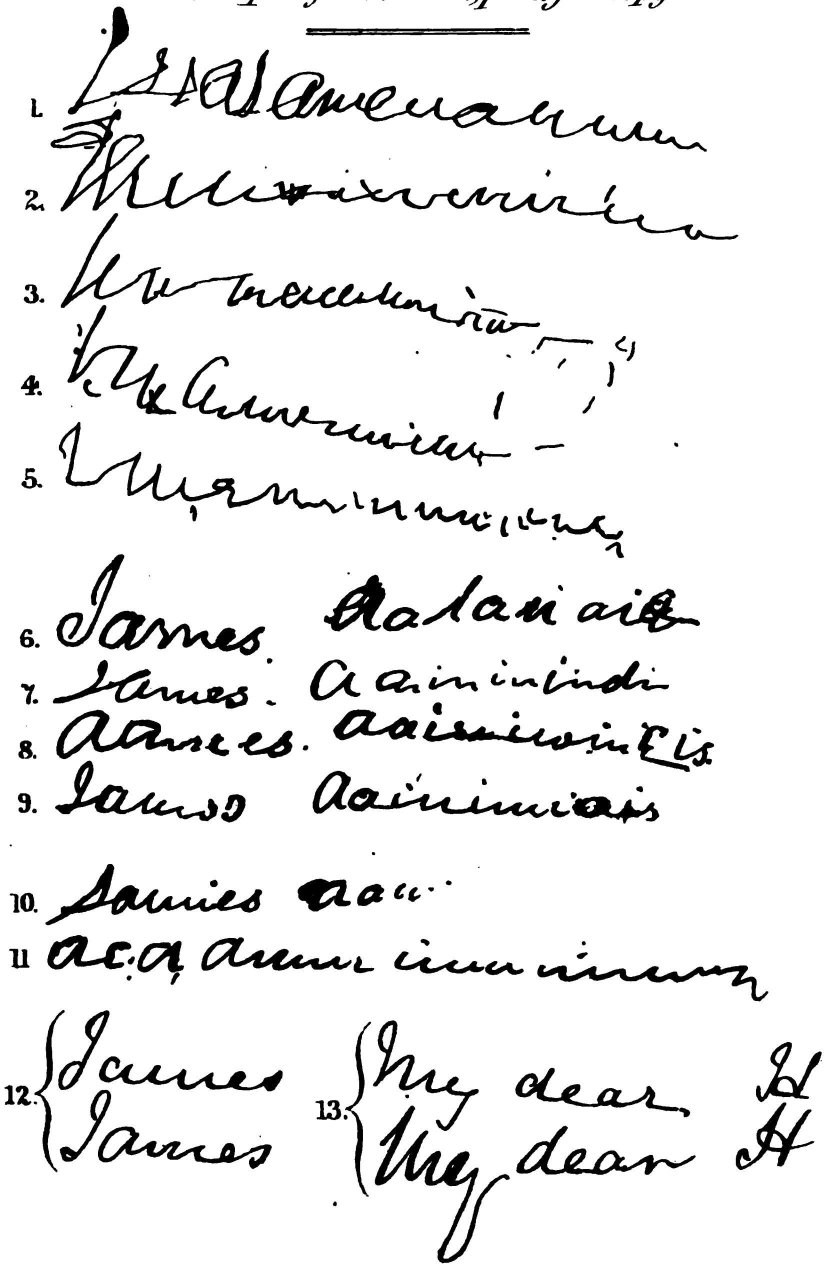
The few accurately observed facts that we have as yet regarding the writing of the aphasic correspond, in the main, with the theory above mentioned. Notwithstanding some noteworthy exceptions (to appearance) in which aphasic individuals are said, on rather good authority, to have been capable of expressing their thoughts in writing, the usual fact is certainly the opposite; the aphasic writes at least as badly as he speaks; and when he speaks not at all, he also writes not at all. Two circumstances interfere with the precision of this observation, as now stated: viz.,—1. The fact that in aphasia there is most commonly paralysis of the writing hand; and 2. The fact that there may be in real or supposed aphasia a complication with paralysis of the organs of articulation, which, again, may leave the writing hand free to express the thoughts. Cases have certainly been observed of apparently aphasic individuals who have been able to write with the left hand, and in some instances with the paralyzed hand, so as to be distinctly followed, and even to transact business; but the cases of the opposite condition are to my mind so distinct and conclusive that I am driven to one of two suppositions (in the present state of information), to account for the exceptional cases;—either the alleged cases of aphasia with ability to write were not *true* aphasia, but cases of sudden and complicated paralysis of the organs of articulation; or we must admit *two perfectly distinct* kinds of aphasia, only one of which affects the *ideation* of language, so to speak, while the other affects, in some complicated way as yet imperfectly studied, but perhaps differing from paralysis properly so called, the *innervation* of language, or rather of speech, while it leaves the *ideation* of it, on the one hand, and the mechanism of it through the writing hand, on the other, absolutely or nearly intact. Dr. Sanders and others compare this latter class of cases with the much better known "writer's cramp," in which the speech is unaffected, but the power of writing gone. I will not pursue this doubtful

[redacted]

Proceedings of the Philosophical Society of Glasgow, 1865-66.

Specimens of Aphasic Handwriting,

written partly to dictation, partly to copy.



question farther at present, observing only that I should be disposed to regard *true* aphasia as necessarily involving the *idea* of language, and therefore striking at the root of written as well as spoken language; and there are quite enough of cases of this kind on record to show that the fact is sometimes so, and therefore to justify us in either limiting the definition of aphasia, or subdividing it absolutely into two different species, differing not only in degree but in kind. I am able here to present you with a specimen of the handwriting of an aphasic individual of the first kind—that is, in whom the *ideation* of language was affected. The handwriting speaks for itself better than I can do; but I wish, while it is going round, to explain at this stage more exactly what I mean by the *ideation* of language, and to submit the term, with all possible diffidence, as subject to the correction of friends more versed in philosophy and psychology than myself. *The thought or idea which tends towards verbal expression* was not wanting in this gentleman; I have the most clear and multiplied proofs of that fact, from circumstances, some of which have already been stated in a former part of this paper. *Ideation* in general, then, if you admit such a process, was practically undisturbed; nor was the mechanical function necessary to guide the pen to the formation of letters wanting, as you will presently be convinced. But between the two—between the idea *that strives to be embodied in a word* and *the word itself*—there was a gap which this person could not overleap to more than a very limited extent; and he could no more overleap it in speech than in writing, or in writing than in speech. The two kinds of effort on his part led to precisely the same conclusion as to his mental state,—viz., that although the idea tending towards expression was there, and the mechanical power of co-ordination of muscular effort necessary for speaking and writing was there also, there was something wanting between the two; and this missing link was, I think, *the idea of the word itself, considered merely as a symbol of thought,* apart both from vocal expression and from written expression. Now, observe how completely expressive this handwriting is, in connection with what I have presently to tell you of the state of mind I have now endeavoured to define. There is no doubt at all as to what this person was *intending* to write on this page—no doubt that he was seriously and deliberately trying to express his own name and that of his sister in these five lines of straggling incomprehensible hieroglyphics. (See Plate II., No. 1 to No. 5.) You may try, but you will not succeed (for I have tried the experiment repeatedly with various persons) in discovering the name of my poor friend. Were it otherwise, I should not, of course, be anxious to expose this little record of

him to your view. I shall not, of course, tell you what his name was. But I will say that in the five lines here submitted, I can only just detect here and there the merest *trace* of the name; in one of them something a little like the initial letters correctly placed, and in one transposed. It is plain that there has been a *search* after the name in the depths of the mental consciousness, and with a strong conviction that it ought to come, but it has not come,—no, not so much of it as that any of you can possibly guess anything like what it really is. I will tell you something more about the name, nevertheless. The Christian name is James. One of the two other names begins with A. Now, with the help of this you will trace, I think, both J and A, and perhaps you will have an inkling of the third initial letter. But of the rest of the name you will make nothing whatever, and, for reasons above referred to, I shall tell you no more about it.

Observe, however, this further characteristic of the writing before you. In one or two parts of it there is a strange tendency to repetition of the letter *A*. In the first line there is apparently a violent effort after a *J* to begin with, which you can trace as breaking down ineffectually more than once, and then comes a scratch, and then an *A*, pretty well written, and then an *S*, or perhaps an attempt at a *J* again, and then another *A*, and after that various unintelligible hieroglyphics, in which the letter *a*, however, comes in again once or oftener. In the second line there is a still worse attempt at a *J* than in the first, and then unintelligible characters with something like an *a* in the middle, and another, pretty distinct, at the end. In the third line there are two *A*'s at the beginning following one another, and perhaps one or two others in the midst of the hieroglyphics. In the fourth there is a large and well-formed *A*, which ranks about the third letter, and traces of others. In the fifth there is scarcely anything recognizable at all, unless it be a *J* at the beginning, with up and down strokes which may stand for *m* or *n* all through. I told you that there was an initial *A* in one of the three names. Besides this you will see one or two attempts to dot an *i*; and there is a small *i* in the name. The scratches are not therefore perfectly arbitrary; they are obviously attempts, and persistent, one would say obstinate attempts, to force a way through difficulties which are found practically insuperable, into clear expression; and they contain traces of the lost faculty sufficient, I think, to lead us some way towards a conclusion as to what it was that was lost. This comes out still more clearly from other specimens of this poor gentleman's handwriting, which I have in my possession, but cannot exhibit to you so completely as these, on account of their telling more than it would be right for you to know. A few

selected fragments, however, are shown upon the lithographic plate in facsimile, and of these you may assume that wherever the writing is in the least degree intelligible, it was, in point of fact, written to a copy. (See Nos. 6-13.) Indeed, several of the specimens before me show that even when something like distinctness was attained in this way, the removal of the copy was followed almost immediately by a relapse into chaos. The watchful and kind relatives of this gentleman tried, in fact, to teach him to write, until it was clear that the effort was of no use, and he became himself discouraged, and would try no more. In the course of these attempts the *intoxication* (so to speak) of his brain with the letter *A* is very apparent. Thus he begins the *James* with *Aa* on several occasions, ending either with unintelligible characters or with something like the real ending. The second name he almost always begins with an *A* or *Aa*, and the third name frequently in the same way. And these specimens were mostly written under the very eye of the tutor, and to dictation, letter by letter, of one who all but held the pen for him. At last we have a crowning triumph. He actually writes his own name and that of one or two of his relatives; but observe how it is done. The name is set for him as a copy, in plain and distinct characters, and he copies it much as you would copy Hebrew, or Sanscrit, or Chinese, if you only knew that there were letters there, and that they had to be imitated stroke by stroke; or rather, perhaps (to speak strictly according to the facts), as a man might be supposed to copy a language which he had once known how to write, but had almost entirely forgotten; the hand retaining, as it were, a certain amount of mechanical skill independently of the brain. The handwriting so produced, however, is very legible, and even bold. It is not at all like the cramped characters you see in the first samples, and is a complete answer to the notion which might otherwise be suggested, that there was a paralysis of the writing faculty as regards the mechanism. The real state of the case obviously is, that he could write perfectly well *to a copy*, whenever he could be got to take the necessary trouble, but he could not *think writing;* he could not, even to the extent of two or three letters together, connect the *symbol* with the idea symbolized in a word, and all attempts to make him do so resulted in a senseless repetition of *Aa*, with an occasional attempt at a *J* or an *i* or an *m* or an *n*, and a multitude of meaningless scratches.

What makes all this much more curious is, that the gentleman here referred to, who could not put three letters together in writing, and who could only speak by snatches of half-intelligible sentences; who could not, moreover, read a single line of any other person's writing, so far as we could discover, either in print or in MS.; who was

therefore, as utterly bereft as it is possible to be of the faculty of using writing to any useful purpose,—was quite acutely conscious of the delicate and almost indescribable resemblances between one handwriting and another! It became plain to us, indeed, in the course of our observations, that he appreciated very exactly certain special characters of handwriting as a piece of mere penmanship; while, as a *legible* symbol, and as an index to ideas, all handwriting whatever was to him a mere mass of unintelligible ciphers. This is certainly a most strange fact, and you will be justified in expressing a doubt of it. I had, however, the following odd opportunity of verifying the fact, the more convincing to me that it was wholly unexpected. I give this anecdote to you exactly as it was told to me. A letter came from his younger brother, who was in business, and whose handwriting was becoming confirmed and strengthened by business—passing, in short, from a schoolboy's hand to a business hand. In this transition it was becoming liker than before to the original handwriting of our aphasic patient. The letter was put into his hand by his mother, and as soon as he looked at it, his face lighted up as if surprised, and he handed back the letter, with the exclamation —"Why, it's myself!"

It is worth while to note, also, that this gentleman was once caught poring over a share list which had been put into his hand, with the view of testing his power of appreciating numbers, as he had been a very good accountant. Generally speaking, it was of no use; his mind was closed to the symbolism of numbers, as much as to the symbolism of words. On one occasion, however, in a comparatively lucid interval, he fixed his eyes on a complicated figure, say 2764, and ejaculated once or twice, "Two thousand—two thousand—quite absurd!" He had apparently guessed at the general character of the number, but could go no farther than two thousand in detail. But though he could not read 2764 correctly upon a sheet of paper, this gentleman could read off the marks upon a set of dice, and play the corresponding moves upon a backgammon board, without any considerable error for games together.

Have I conveyed to you, through these details, any more clear conception than before of the state of aphasia? for I confess that amidst the warm interest I took in this poor gentleman's case as a friend, and as his physician, it appeared to me all along that the facts had an interest beyond the individual; and I am therefore very solicitous to present them to your minds just as they occurred, and with only so much of theoretical explanation as is almost necessary in observing all complex facts, and such as was present to my own mind in making the

investigations of which you have here a short account. Beyond this I desire to leave the matter in the hands of the metaphysicians, and to hold myself only responsible for a faithful narrative.

Of the actual state of mind of the aphasic we have indeed only very imperfect revelations. And the reason is evident: the destruction of the leading channel of intercommunion of minds places an almost insuperable obstacle in the way of an understanding. It is easy, indeed, to ascertain that the aphasic patient has preserved, to a certain extent, his reason, understanding, emotions, will; but to what extent we can almost never know. The man affected with this strange infirmity is almost of necessity self-centred and alone in the midst of the crowd. Were he a poet of the rank of Milton, for instance, he must needs be a "mute inglorious Milton." Were his soul as sublimely moved as that of the Psalmist, he must be content to "meditate in the night-watches," to "commune with his own heart upon his bed, and be still." There is something, I think, strangely and beautifully pathetic in this; and I am tempted to expand the idea a little, even at the risk of departing for a moment from my programme. Can we find a better meditation for such a being than these passages of the 77th Psalm? and is there anything in his state which should prevent his remembering and adopting them,—not in words, it is true, but in substance?—"In the day of my trouble I sought the Lord . . I remembered God, and was troubled: I complained, *and my spirit was overwhelmed.* Thou holdest mine eyes waking; *I am so troubled that I cannot speak.* I have considered the days of old, the years of ancient times. I call to remembrance my song in the night, and my spirit made diligent search. Will the Lord cast off for ever, and will he be favourable no more? Is his mercy clean gone for ever? Doth his promise fail for evermore? Hath God forgotten to be gracious? Hath he in anger shut up his tender mercies? And I said, This is my infirmity; but I will remember . . the works of the Lord; surely I will remember thy wonders of old." I beg you to believe that I have not made this quotation merely to round a paragraph. I have made it in all seriousness and deliberation, and for the purpose of leading you to consider the question—Could an aphasic patient think thus? Could his thoughts reach thus high? Could his feelings be thus warm and vivid? Could he in the night-watches even *meditate* such an outpouring of fervent pathos as this?

For my part, I believe so; or at least I have seen nothing as yet, in my considerations on the mental state of the aphasic, to lead me absolutely to exclude such a supposition. But we are not left entirely to hypothesis in this matter. A few cases of very temporary but very

complete aphasia have been recorded authentically by the sufferers themselves after their recovery; and one of these is so curiously apposite to the solution of the question raised above, that I shall venture upon giving you the whole case, as quoted by the late Dr. Cheyne,* of Dublin, from an all but forgotten source in the periodical literature of the last century.

"In the third volume of *The Hygeia*, by Dr. Beddoes, the very curious and well-known case of Dr. Spalding, of Berlin, is quoted, and erroneously referred to mere hurry of ideas preceding epilepsy. On the 31st of January, 1772, he had to speak to many people in quick succession, and to write many trifling memorandums concerning very dissimilar things, so that the attention was incessantly impelled in contrary directions. He had at last to draw out a receipt for interest; he accordingly sat down and wrote the first two words requisite, but, in a moment, became incapable of finding the rest of the words in his memory, or the strokes of the letters belonging to them. He strained his attention to the utmost in endeavouring leisurely to delineate letter after letter, with constant reference to the preceding, in order to be sure that it suited. He said to himself that they were not the right strokes, without being able in the least to conceive wherein they were deficient. He therefore gave up the attempt, and partly by monosyllables, and partly by signs, ordered away the man who was waiting for the receipt, and quietly resigned himself to his state. For a good half hour there was a tumult in part of his ideas. *He could only recognize them for such as forced themselves upon him without his participation.* He endeavoured to dispel them to make room for better, which he was conscious of in the bottom of his thinking faculty. He threw his attention, as far as the swarm of confused intruding images would permit, on his religious principles, and said to himself distinctly, that if by a kind of death he was extricated from the tumult in his brain, which he felt as foreign and exterîor to himself, he should exist and think on in the happiest quiet and order. With all this there was not the least illusion in the senses. He saw and heard everything about him with its proper shape and sound, but could not get rid of the strange confusion in his head. He tried to speak, for the sake of finding whether he could bring out anything connected; but however vehemently he strove to force together attention and thought, and though he pro-

* *Essays on Partial Derangement of the Mind in supposed connection with Religion*, by the late John Cheyne, M.D., &c., Physician-General to His Majesty's Forces in Ireland. (A posthumous work, now long out of print, but well worthy of re-publication.) Dublin, 1843. Essay 3, "On Disorder of a Single Faculty."

ceeded with the utmost deliberation, he soon perceived that unmeaning syllables only followed, quite different from the words he wished. He was as little master now of the organs of speech as he had before found himself of those of writing. 'I, therefore,' says he, 'contented myself with the not very satisfactory expectation that, if this state should continue, I should never, all my life, be able to speak or write again; but that my sentiments and principles, remaining the same, would be a permanent spring of satisfaction and hope, till my complete separation from the unfortunate ferment of the brain. I was only sorry for my relations and friends, who, in this case, must have lost me for duties and business, and all proper intercourse with them, and looked upon me as a burden to the earth. But after the completion of the half hour, my head began to grow clearer and more quiet. The uproar and vividness of the strange, troublesome ideas diminished. I could now carry through my process of thought—I wished now to ring for the servant, that he might request my wife to come up. But I required yet some time to practise the right pronunciation of the requisite words. In the first conversation with my family, I proceeded for another half hour slowly, and in some measure anxiously, till at length I found myself as free and clear as at the beginning of the day, only I had a very trifling headache. Here I thought of the receipt which I had begun, and knew to be wrong. Behold, instead of fifty dollars for half a year's interest, as it should have been, I found in as clear and straight strokes as I ever made in my life—"*fifty dollars through the sanctification of the bri-*" with a hyphen, as I had come to the end of the line; I could not possibly fall upon anything in my previous ideas or occupations which, by any obscure mechanical influence, could have given occasion to these unintelligible words.'"—pp. 94-97.

Dr. Cheyne's remarks upon this singular narrative are extremely interesting, and remarkably in accordance with all the more recently observed phenomena of aphasia. They are as follows:—

"The case just described serves well to illustrate the suspension of that faculty by which thought is communicated by speech or writing. Spalding had been engaged in a way not to exhaust or perplex his mind. The memorandums be had been making related to things of no great importance, and his attention had not long been on the stretch, having been relaxed by the dissimilarity of the concerns with which he was occupied. During the attack he was in possession of all his faculties save one. The confusion of mind may be ascribed to alarm, lest he should never again be able to communicate with his friends. In explaining his feelings, he adopts a very extraordinary theory of mind. He says he endeavoured to expel his tumultuous ideas, to

make room for better, which he was conscious of in the bottom of his thinking faculty. But, in truth, his thinking faculty, notwithstanding his apprehensions, was in a state of integrity. He had an act to perform, which he was as incapable of as of flying to Potsdam; and he felt that had he persevered in the attempt to accomplish it he would have exposed himself to the suspicion of insanity, which, by looking inwards, he knew was not threatened; for not only was there no illusion of the senses, but his principles, sentiments, and affections were unaltered. How then did he act? With the greatest presence of mind: he got rid of all witnesses, that by rest and quiet he might regain that equanimity which is in general so conducive to fluency of expression; nor did he admit his friends till he had proved to his own satisfaction that he could once more communicate with them on an equal footing. As has been conjectured, Dr. Spalding was threatened with epilepsy."—pp. 97, 98.

I cannot give these extracts from Dr. Cheyne's admirable work, without remarking that, in his philosophy of this difficult subject, and also in the selection and narration of instances, he appears to me to have anticipated almost the whole course of modern research, with the exception of the anatomical facts bearing on the localization of the faculty in the brain. He defines the special disorder which has since received the name of aphasia as "an interruption to the power of expressing thought, even when the mind is in other respects unimpaired." In reference to the views of Sir Alexander Crichton, who, in his work on *Mental Derangement*, has placed these cases under the head of disorder of the memory, Dr. Cheyne observes, that "while the individual is deprived of the power of communicating his recollections, his memory is retentive with respect to persons, places, events, and their order"—a criticism which is at once an evidence of the accuracy with which Dr. Cheyne had observed this condition, and a demonstration of the carelessness of some more modern authorities, who, even in citing Dr. Cheyne's instances, have failed to apply the searching and logical inductions by which they are accompanied.

Other instances, bearing more or less accurately on the state of the mind in aphasia, are not wanting. A much-respected friend of mine, learned in the law, as well as in other things, and who was never, even for a moment, suspected either of epilepsy or insanity, or any other infirmity of the brain, told me only the other day of a nearly similar but much more evanescent, and therefore trifling, manifestation of aphasia which has repeatedly occurred to himself. When a young man he was subject to a sudden form of vertigo or dizziness, unattended by headache or sickness, but perhaps more allied to what is usually

associated with this in the disease called "sick headache" than to any other disease in the nosology. At a time when he had no reason to expect it—*e. g.*, when at dinner, or in his study, or with his family, and quite undisturbed by emotion or otherwise—he would observe a flickering before his eyes, as if a sudden steam or mist had arisen, or, as he describes it, like what takes place when you look through an atmosphere in which a hot and cold current of air meet each other; and this affection recurring again and again (though lasting only for a few minutes or even seconds), he came at last to view almost as a settled and familiar infirmity, which he had only to study and care for, in so far as it might tend to interrupt him when engaged in important business. Viewing it in this light, he was particularly struck with one fact, viz.,—that whenever he attempted to speak in the midst of this vertigo, the words seemed to come out quite differently from what he intended, and as if he had lost all control over them. He had indeed no conception of what he was saying, though his mind never lost hold of what he *meant to say*. In fact, he talked nonsense, knowing and feeling that it was nonsense, but without the least power to make sense of it. And from the frequent recurrence of this state he came to know that whenever this vertigo came on, *his only safety against talking nonsense was in absolute silence*, and this he accordingly maintained. He is still subject to the vertigo, but without the concomitant affection of the speech; and he is quite certain that he never even for a moment lost his senses, or was in any way permanently injured for business or otherwise, in any of these attacks.

Even as I write, another illustration occurs of temporary aphasia concurring with perfectly uninterrupted consciousness of the abnormal state of the power of expression. A gentleman aged more than seventy, who had once or twice suffered from a threatening of paralysis on the right side, sat down the other day to write a business letter, and found that he could not get on. He tore up the letter, and next day went to call upon his medical attendant in the country, telling him, among other things, and amid considerable confusion of utterance, of his difficulties with regard to the letter, which he had torn up because he "could not make nonsense of it." A few days afterwards he tried to write a letter to his daughter, and wrote quite according to his mind into the second page, when, his attention becoming fatigued, the characters suddenly became unintelligible, and he carried the letter, with tears in his eyes, to a friend, expressing his extreme annoyance at not being able to finish it.

There are two instances recorded in France, and given in M. Trousseau's elaborate essay, in which men of high mental capacity, and

able, more than most, to reason upon their own impressions, have passed through attacks of aphasia from which they have recovered, and thus become the recorders of their own cases.

The first of these cases was that of one of the most eminent members of the Faculty of Medicine in Paris, a man who had made a careful and special study of the diseases of the brain. This eminent man, after a days' hard reading, was perusing one of Lamartine's essays, when he suddenly became aware that he had failed to follow with his mind what he read. After a renewed unsuccessful attempt to read, he became frightened, and wished to call out, but could not utter a word. He then began voluntarily to move all his limbs in the most complicated manner, to convince himself that he had no paralysis. He rang the bell, but could not speak a word to the servant; he satisfied himself by moving his tongue in every direction that there was no paralysis there. He then made a sign for pen and ink, but found that he could not write any more than he could speak. All this time he was mentally engaged in discussing and analyzing his own symptoms, trying to refer them to their probable cause within the brain, and generally viewing his own case exactly as he would that of another at a clinical lecture. On the arrival of a physician two or three hours afterwards, he presented his arm, turned up his shirt sleeve, and made signs that he wished to be bled. The bleeding was scarcely over, before some words, unconnected it is true, and incomplete, returned; gradually the mist cleared away, and after about twelve hours, he was able to speak and write as well as ever.

The case of another eminent teacher of medicine, "the illustrious Professor Lordat," of Montpellier, is mentioned by M. Trousseau, but with a certain disposition to demur to his conclusions, and even his statements, on the ground that M. Lordat, "in virtue of his spiritualistic (*i. e.* idealistic) doctrines, believes in the complete independence of thought and speech, and *a fortiori* in the independence of thought and of the organs of speech." Setting aside this metaphysical prepossession, which I think need hardly be supposed to have interfered with a question of fact, it appears that M. Lordat in the aphasic state was able to think, to arrange the materials of a lecture, and to change the distribution of them; while neither by speech nor by writing was it possible for him to communicate an idea; and this although there was no paralysis. "I reflected," he writes, "on the Christian doxology, 'Glory be to the Father, and to the Son, and to the Holy Ghost,' and it was impossible for me to *recall even a single word of it.*" The thought remained intact, but the power of expression was gone. At the same time he convinced himself that he could combine abstract ideas, and distinguish them

quite well from each other, *without having a single word to express them, and without in the least degree thinking on the expression of them.* "I experienced," he adds, "no embarrassment in the exercise of thought. Accustomed as I was for so many years to perform the work of a teacher, I congratulated myself on being able to arrange in my head the principal propositions of a lecture, and on finding no difficulty in changing the order of ideas as I pleased." It appears, nevertheless, that M. Lordat, after his recovery from this attack in 1828, had lost to a considerable extent the power of lecturing without notes, which he possessed in perfection before; and on this rather slender foundation (as it appears to me), M. Trousseau builds up a theory opposed to that of M. Lordat, and even contends that the latter was mistaken or biased in his narrative of personal experience, by his theories. In this respect I think the distinguished professor of the Hotel-Dieu is probably unjust to his colleague's statements, or at least goes further in the way of criticism of them than his own observations warrant; for there is no difficulty to a mind not unduly occupied with metaphysical theories in admitting that a certain amount of deterioration of the thinking faculty may probably follow a lesion of the brain involving speech, just as it may follow a lesion of the brain involving sight, or motion, without any presumption thence arising that thought and speech are identical, or even inseparable.

As far, therefore, as the mere *absolute essence* of the aphasic state is concerned, I am disposed to hold with Lordat rather than with Trousseau. Without asserting that the elements exist for a final judgment, I think strong evidence has been given that in the simple form of aphasia, attacking with great suddenness a previously healthy and intelligent man, there is not *necessarily* any immediate diminution of the general intelligence whatever. But farther than this I cannot go. I fear it must be admitted that in the great majority of aphasic cases there is a loss of general intelligence; not only because the affection is usually combined with paralysis and with more or less complicated disease of the brain, but also because I find it difficult or impossible to conceive of a state in which the symbols of thought shall be permanently lost, and thought itself sustain no injury. It is a law of nature which, so far as we know, is without exception, that all our faculties are developed by use, and dwarfed by permanent inaction. The muscle which ceases to be used most surely undergoes degeneration and atrophy; the optic nerve which is shut out for a long time from the light shrivels and decays; the very habits which even in small matters grow upon us from day to day often tend to produce in us narrow, hard, individualized lines of thought and action, which, if they develop some of

our faculties, tend to freeze up others, and finally to paralyze much of what is good in all of us, unless we keep the current of thought and emotion flowing by careful tending of the inner man: how should it be but that a being cut off from the use of the chief symbol of thought should degenerate in the end as regards thought itself? The complete aphasic is, indeed, in a worse condition than the deaf and dumb; for they at least can *think* words, and, with the help of a special education, can express them within a limited circle; whereas he must remain ever wrapped up in himself—a creature incapable of any intercourse with his fellow-men which is founded upon the *idea* of a word. The deaf and dumb can be educated, because they have at least the *idea* of the symbol, and it can be changed into a form accessible to their minds, and capable of being learned and used by them notwithstanding their bodily defect; the aphasic, I think, supposing the disease congenital, could not possibly be educated, but must remain almost an idiot—the mind of an infant enclosed in the shell of a man.

Have we actually seen such cases of undeveloped humanity in which the primary defect has been aphasia? This we know not; probably we never can know. It is far from improbable that certain forms of cretinism, or of congenital idiocy, may be due to an affection of this kind in very early infancy, rendering the development of the mental faculties an impossibility. But surely the condition of the educated being who falls into aphasia by accident or injury, demands a most careful consideration from his fellow-men, as regards the conditions under which he may be admitted to the common rights and privileges of humanity, and the extent to which he may be rendered, by artificial culture and new methods of communication, capable of availing himself of these. This branch of the subject of aphasia is as yet in a very unformed state, and I hesitate to commit myself to any well-defined doctrine upon the subject. I should hope that one effect of these few and imperfect remarks, made in so wide and intelligent a circle as this, may be to bring to light additional facts and observations which may tend to the elucidation of whatever remains dark and mysterious.*

* I have to regret the omission, due to insufficient time for reading during the preparation of this paper for the Society, of a reference which I should for many reasons have wished to introduce into the text. A remarkably interesting narrative of a case of aphasia connected with spectral illusions, and with disease of the left side of the brain, will be found in Vol. xlvi. of the *Edinburgh Medical and Surgical Journal*, October, 1836, page 334. Mr. Craig, of Ratho, has in this case furnished a multitude of details in no degree inferior in interest to those of any case since recorded, and remarkably in accordance with most of the facts stated in this paper. Dr. Craigie has also added some valuable remarks.

Note on the Primary Roots, or Nuclei, of Articulate Speech.

In reference to a question raised in the course of the discussion on this paper, as to the relative position of nouns and verbs, properly so called, among the first roots of language, I will venture another quotation from Max Müller:—"We must not forget that there are languages . . . . in which there is to the present day no *outward* distinction between a root and a word. In Chinese, for instance, *ly* means to plough, a plough, and an ox, *i.e.*, a plougher; *ta* means to be great, greatness, greatly. Whether a word is intended as a noun, or a verb, or a particle, depends chiefly on the position which it occupies in a sentence. In the Polynesian dialects almost every verb may, without any change of form, be used as a noun or an adjective; whether it is meant for the one or the other must be learnt from certain particles, which are called particles of affirmation (Kua), and the particles of the agent (Ko). In Egyptian, as Bunsen states, there is no formal distinction between noun, verb, adjective, and particle, and a word like *an'h*, might mean life, to live, living, lively. What does this show? I think it shows that there was a stage in the growth of language, in which that sharp distinction which we make between the different parts of speech had not yet been fixed, and when even that fundamental distinction between subject and predicate, on which all the parts of speech are based, had not yet been realized in its fulness, and had not yet received a corresponding outward expression," *l. c.*, pp. 84, 85. The subject is of some importance in reference to the details of the preceding paper; and although I cannot entirely adopt, at least without some qualification in the mode of statement, Professor Müller's views as to the essential identity of thought, or reason, and speech (λόγος), there can be no doubt that the very derivation of the word *noun*, from *nomen*, ὄνομα, νοέω, νοῦς, whence also *nosco*, I know, carries back to a remote Sanskrit or pre-Sanskrit root the whole idea of *naming* or *knowing* things (whether objects of sense or ideas of the mind), as being closely conjoined or associated operations of the νοῦς. Not to pursue the etymological argument too far, however, it may be admitted that anything to which we can distinctively attach a name has first to be fixed as a general conception in the mind, apart from particular instances. When I use the word *horse*, for example, I think not of one particular horse, but of the general idea of a horse as present to the mind in a survey of the distinguishing qualities of the animal. When I use the word John, I have risen from the conception of any particular object or man actually observed in connection with that word, to the more general point of view of John as the name of many men. And so when

I discriminate a certain emotion, and name it *love*, I place the idea of love before my mind in a general form, which may afterwards be applied either as a verb active, *e.g.*, *I love*, or as a noun substantive, *e.g.*, *love is present within me.* The question of precedence is difficult to determine, and no one, I think, will consider that I am unduly trespassing on the patience of the reader in adding the following extremely beautiful illustrations from the same rich mine of thought from which I have already quoted above. "To a Chinese, the sound *ta* (great), even when pronounced, is a mere root; it is neither noun nor verb, distinctions which, in the form in which we conceive them, have no existence at all to a Chinese. If to *ta* we add *fu* (man), and when we put *fu* first and *ta* last, then, no doubt, *fu* is the subject, and *ta* is the predicate, or, as our grammarians say, *fu* is a noun, and *ta* a verb; *fu ta* would mean 'the man is great.' But if we said *ta fu*, *ta* would be an adjective, and the phrase would mean, 'a great man.' . . . . If we watch the language of a child, which is in reality Chinese spoken in English, we see that there is a form of thought and of language perfectly intelligible to those who have studied it, in which, nevertheless, the distinction between noun and verb, nay, between subject and predicate, is not yet realized. If a child says *up*, that *up* is, to his mind, noun, verb, adjective, all in one. It means, 'I want to get up on my mother's lap.' If an English child says *ta*, that *ta* is both a noun, thanks, and a verb, I thank you. Nay, even if a child learns to speak grammatically, it does not yet think grammatically; it seems, in speaking, to wear the garments of its parents, though it has not get grown into them. A child says, 'I am hungry,' without an idea that *I* is different from '*hungry*,' and that both are united by an auxiliary verb, which auxiliary verb again was a compound of a root *as*, and a personal termination *mi*, giving us the Sanskrit *asmi*, I am. A Chinese child would express exactly the same idea by one word *shi*, to eat, or food. The only difference would be that a Chinese child speaks the language of a child, an English child the language of a man."—Ibid., pp. 85, 86.

[In the course of his paper, and at its close, Dr. Gairdner briefly remarked upon the curious anatomical facts adduced by MM. Dax, Auburtin, Broca, and others, as to the connection of aphasia with structural disease of the left hemisphere of the brain, and especially with a particular portion of the left frontal lobe. Dr. Sanders had observed and recorded a case bearing on this department of the inquiry, and he (Dr. Gairdner) had lately seen another; but it was more in accordance with the plan of his paper, and at all events more likely to be agreeable to the Society, considering the great length of time he

had occupied their attention, to leave that part of the subject to Dr. Sanders and Dr. Allen Thomson, who, he trusted, would take part in the discussion. So far as his (Dr. Gairdner's) opinion went, it was generally in accordance with the views of the observers above-mentioned.]

In answer to the invitation of the President,

Dr. Sanders, of Edinburgh, said he had come merely to listen, and was not prepared to speak on the subject, but as Dr. Gairdner had referred to him in connection with the medical aspect of the inquiry, he would notice it very shortly. The subject of aphasia, or speechlessness, was a very old one, and was known in general literature. It would be remembered that Zacharias was speechless until St. John was baptized; and other similar instances were recorded in ancient writings; but it was more especially within this century that this affection of speech had been studied scientifically. The phrenologists were the first to take up the investigation in relation to the localization of certain faculties in definite parts of the brain. And although their theories had not been generally accepted, yet they had induced physicians to inquire whether particular portions of the brain were disordered when defects existed of particular faculties of the mind. These inquiries had not yet led to any positive result, or to anything which we could be assured would ultimately stand; but enough was known to show the necessity of further investigation. So far back as 1825, one of the leading physicians in Paris, M. Bouillaud, had asserted, as the result of his observations, that in patients who had lost the faculty of articulate speech, disease was invariably found in the anterior lobes of the brain. This remarkable statement, though controverted by other eminent authorities, had been confidently maintained by its author up to the present time. But within the last five years, viz., in 1861, much interest had been again excited on the subject in Paris, in consequence of the discussions at the Anthropological Society, one party (M. Auburtin, especially) maintaining, while another party (M. Gratiolet, among others) denied the localization of certain faculties, such as language, in special parts of the brain. It happened at the time of this discussion that there were two cases of aphasia under the care of M. Broca at the Bicêtre, and that gentleman asked M. Auburtin and others to attend the *post mortem* examination of these patients, so as to test by crucial instances whether the brain was affected in its anterior lobes. They found that the disease was not only in the anterior, but in the left anterior lobe, and occupied exactly the external left frontal convolution, where the anterior lobe meets the middle lobe, immediately in front of the fissure of Sylvius. From the evidence of these two cases,

the proposition was advanced by M. Broca, that the faculty of language, so far as articulate speech was concerned, must have its seat in that particular part of the brain. This was M. Broca's opinion, and there were a number of cases in support of it, but it must also be admitted that other cases, opposed to M. Broca's views, threw difficulty in the way of its acceptance. It was a question, therefore, that must be tested by future observation, and as the disease causing aphasia was not necessarily nor speedily fatal, these difficulties could not quickly be set at rest. Dr. Sanders had had, since August last, four cases in the Edinburgh Royal Infirmary, one of which recently proved fatal, and accordingly it was carefully investigated whether the disease would be traced to the particular part of the brain already indicated; and, strangely enough, it was found in the exact spot. That was a remarkable coincidence, and to many persons it might at first sight appear nearly decisive of the point; but medical men knew very well that it was not safe to found on particular examples, however striking. He held that it merely showed that there was a case for inquiry, to ascertain whether it were possible to find out the position of certain lesions of the brain connected with loss of speech, or whether the loss of that faculty was merely one indication, among others, of cerebral disease in general. So much for the technical part of the question; but the circumstances attending aphasia were of great curiosity and interest, in a general point of view, in relation to metaphysics and the philosophy of mind, as Dr. Gairdner has ably indicated in his paper. There was a great deal of plausibility, but there was nothing more, in the facts advanced to prove that particular faculties of the mind were connected with particular parts of the brain, or act in connection with those parts of the brain. As yet the connection of the mind with the brain cannot be scientifically determined, nor even accurately surmised. Dr. Sanders had no doubt that the discussion of the metaphysical parts of the question would present valuable results. The symptoms in aphasia bore directly on the questions, how far ideas might be thought out by other symbols than words; and what the relation was between the thought and its signs, whether these were speech, writing, gesture, or other modes of expression.

Dr. Allen Thomson agreed with Dr. Sanders as to the great interest of the subject brought before the meeting, both in its physiological and its philosophical aspects, and thought that the thanks of the Society were due to Dr. Gairdner for the very able and instructive exposition he had given of the phenomena of aphasia, and to Dr. Sanders for the very interesting supplementary remarks which he had made. It appeared to him from what had been referred to by

these gentlemen, and from what he had himself observed many years ago, when his attention was called to the phenomena of varying degrees and forms of muteness, that it might be regarded as extremely probable that certain cases of want of speech depended on a mental or cerebral defect of the power, as it were, to translate ideas into the outward expression of them by language. For, on the one hand, the instances of persons partially mute from an imperfect condition of the organs of hearing, as well as the experience of those engaged in teaching the partially deaf to speak, showed to how great an extent the mere mechanism of speech is dependent upon the imitation of that mechanism in other persons; and on the other hand, the occurrence of cases—somewhat rare, no doubt—of muteness from physical defects of the organs of speech, showed that, notwithstanding the possession of a complete knowledge of language and its relations to ideas, the expression of ideas in speech may be completely interfered with by mere want of the proper machinery of utterance. From both of these classes of cases, however, the instances referred to by Drs. Gairdner and Sanders appear entirely to differ; and the accidental observation of several examples of the same description had many years ago led him (Dr. Thomson) to views somewhat analogous to those which have been more fully established on more extended and accurate observation of the phenomena, and which have been so clearly exposed by Dr. Gairdner in his paper. Indeed, he confessed that the cases of aphasia to which he now referred were to him, at the time, very inexplicable, and the mode of attempting their treatment a matter of extreme difficulty. It might be hoped, however, that now a more correct appreciation of their nature might lead to more rational plans being devised for their cure or amelioration. To him (Dr. Thomson) the study of the muteness of the deaf seemed a most interesting subject. He had seen it in very various degrees, more especially in cases of the closure of the external ear, with more or less integrity of the power of hearing in the inner part, which led to partial deafness, and was attended with various degrees of the partial use of language. In these persons sounds reached the organ of hearing through the hard parts of the head. There was the case of a lad of sixteen years of age who, having both ears congenitally closed, heard best at the top of the head, and who possessed such an amount of power of hearing and speech as to enable him to converse with his playfellows and profit by tuition in school; and various gradations of other cases came before him, in which, with less or greater defect of the middle or internal part of the ear, the persons had a greater and greater inability to make use of language Through these he was led to study

that remarkable relation which existed between the power of putting thoughts into words and the physical expression of ideas, and thence he was led to the study of that curious relation, to which Dr. Sanders had referred, existing between the different forms in which language might find expression and the ideas they were intended to express. It was certainly a most curious subject for consideration, what ideas could be attached to the visible symbols that were made use of by the deaf and dumb. And of still greater interest was the study of such instances as that of Laura Bridgman and others, in whom the sense of touch was the only channel through which they could find an entrance to or outlet from the sensorium. At the end of his paper, Dr. Gairdner had referred to a fatal case, of recent occurrence, in the Glasgow Royal Infirmary, in which the lesion of the brain was in the left side of it, and apparently in the very situation pointed out by the French authors as the spot affected in cases of ascertained aphasia, and he would like to know to what extent the language was affected or interfered with in that case. The disease in this case of Dr. Gairdner's, as in that of Dr. Sanders, was found to be at the meeting of the external frontal with the anterior ascending parietal convolution of the brain, according to the nomenclature of Gratiolet.

Dr. Gairdner said he had purposely not alluded to the details of the case referred to, because it would have made the paper much too long, and because it was not a very easily described case. He was convinced in his own mind that there was aphasia present in the later history of the case; but it would not be very easy to make that clear to the Society within a reasonable length of statement; for when the man was admitted to the hospital he could speak, and it was only when he got worse that he could not speak. But still his (Dr. G.'s) own clear conviction was that there was aphasia, mingled with other symptoms of cerebral disease.

Dr. Allen Thomson remarked that phrenologists placed the organ of language in the part of the brain above the eyes; and hence they conceived it possible to judge by the various degree of prominence or depression of that organ, of the power of language possessed by any individual. But the convolutions in which the phrenologist placed the faculty of language, did not agree with the view resting on the pathological considerations founded on such cases as the present, the affected part being more to the side of the eye, or in the temporal fossa.

Dr. Gairdner.—Besides, the one-sidedness of the disease (the only fact which seems to have been quite clearly made out about it) is opposed to all the calculations of the phrenologists, and was quite overlooked by them in fixing the position of the organ of language.

REV. H. W. CROSSKEY said that since this was one of those subjects in which the medical man and the student of language met, and in which the study of language would throw light upon the study of anatomy, he would remark that naming objects was an exceedingly difficult thing, and involved a process of thought difficult for a child to accomplish; and therefore, it was found that the verb, of all utterances, was the first object, and that to give a name to an object showed a considerable amount of predication and will, so that the arrest of the faculty of the mind to grasp proper names, instead of being an arrest of a primeval quality of the mind, was an arrest of one of the latest and most elaborate actions of it.

The PRESIDENT would not detain the meeting at that late hour by any remarks of his own, but would simply ask them to pass a vote of thanks to Dr. Gairdner for his excellent paper. He could corroborate many of the facts stated in the paper. He had come into contact with several people afflicted by aphasia. One remarkable case he had seen of a gentleman who had lived a busy life abroad, and had come home with a tendency to paralysis. One day when he wanted to go out, he got excited because nobody could understand him. He asked for his "*farflacht*," and the people discovered that it was his greatcoat he wanted; nor could he even be got afterwards to understand that that was not its proper name. He became paralyzed and died. Another case was that of an old lady, who had a servant named Janet, and ever after she became affected she always called her "Parritch."

The cordial thanks of the Society were voted to Dr. Gairdner.

IX.—*On the Estimation of the Evaporative Power of Fuel.* By W. J. MACQUORN RANKINE, C.E., LL.D.

Read April 18, 1866.

1. *Unit of Evaporative Power.*—The most convenient unit of evaporative power, as being a quantity common to the measures of all nations, is the conversion into steam of a weight of water equal to that of the fuel burned, at the mean atmospheric pressure; the feed-water also being supposed to be supplied to the boiler at the temperature of the atmospheric boiling-point.

When the temperature of the feed-water and the boiling-point are

different, the following rule is to be used in order to convert the actual evaporation, as compared with the weight of fuel, into units as above defined:—

For every degree of Fahrenheit that the feed-water is below 212° *add to the actual evaporation one-*966*th* (or ·001035) *of itself;* and *for every degree that the boiling-point is above* 212° *add one-*3220*th* (or ·00031);—or otherwise:

For every degree centigrade that the feed-water is below 100° *add to the actual evaporation one-*537*th* (or ·00186) *of itself; and for every degree that the boiling-point is above* 100° *add one-*1790*th* (or ·00056).

The unit of evaporation at the atmospheric boiling-point is equivalent to 966 Fahrenheit units, or 537 centigrade units, of heat; and to the mechanical work of lifting the unit of weight to a height of 745,800 feet, or 227,300 metres.

2. In researches on the evaporative power of fuel, two objects may be proposed; to find the *total evaporative power*—that is, the number of times its own weight of water that the fuel would evaporate at the atmospheric boiling-point, if all the heat produced by its complete combustion were employed in evaporation; and to find the *available evaporative power* in some actual furnace and boiler: which always falls short of the total evaporative power.

It is on the total evaporative power that the real value of fuel depends; for the available evaporative power in any particular boiler depends not only on the fuel, but on the efficiency of the boiler, and its proper adaptation to the fuel employed. To compare two sorts of fuel which differ much in their properties, by means of experiments with the same furnace and boiler, may lead to very fallacious results; for the same furnace and boiler that are efficient for one sort of fuel may be inefficient for another. Each of the two sorts of fuel ought to be tried in a furnace and boiler specially adapted to it, so that the efficiencies may be the same; and then the actual evaporative powers will bear to each other the same ratio with the total evaporative powers.

3. *Calorimeter Experiments.*—It may be regarded as almost, if not quite, impracticable to find the total evaporative power of any sort of fuel by experiments with a steam boiler: the causes of waste of heat are so many, and their operation so complex. The only practicable method appears to be that of ascertaining the total heat of combustion, by means of a calorimeter, and reducing that heat to the equivalent evaporative power; and to perform that operation with accuracy the following conditions must be fulfilled:—

I.—The fuel must be burned in a closed vessel, completely enveloped

by the mass of fluid by whose elevation of temperature the heat produced is to be measured.

II.—Unless the calorimeter is itself a thermometer, it is essential that the mass of fluid should be constantly stirred by suitable mechanism, in order to insure that the temperature shall be uniform throughout the whole mass, otherwise very great errors may arise. Such errors are likely to be especially great, and to lead to exaggerated views of the value of the fuel, when the only temperature ascertained is that of a layer of fluid at the upper surface of the mass (as when the temperature is inferred from the raising of steam of a certain pressure in a boiler); for that layer is necessarily hotter than any other part of the fluid.

III.—To avoid altogether the risk of such errors, the calorimeter may itself be a large mercurial thermometer, measuring the heating effect by the expansion of the whole mass of mercury contained in it; for that expansion is a correct indication of the *mean* elevation of temperature of the fluid mass, and therefore of the whole heating effect, even although considerable differences of temperature should exist at different points.

These conditions were completely fulfilled by the calorimeters used by Messrs. Favre and Silbermann in their experiments on the total heating effect of the complete combustion of various substances; and hence the confidence placed by men of science in the results of those experiments (*Annales de Chimie et de Physique*, 1852-3).

4. *Results for Carbon, Hydrogen, and their Compounds.*—The following are the results of experiment as regards the total evaporative power of hydrogen, carbon, and their compounds, reduced to units of evaporation:—

I.—The total evaporative power of carbon is 15.

II.—The total evaporative power of hydrogen is 64.

III.—The total evaporative power of any compound of hydrogen and carbon is never greater than the sum of the separate evaporative powers of those fractions of the weight of the compound which consist of hydrogen and carbon respectively. For example, let the compound be olefiant gas, of which seven parts by weight contain six parts of carbon and one of hydrogen. Then,

For the Carbon,..	$15 \times 6 =$	90
For the Hydrogen,....................................	$64 \times 1 =$	64
Divide by	7	)154 sum.
Greatest possible evaporative power of Olefiant Gas,		22

Those three principles having been established, the greatest possible evaporative power of any hydrocarbon can at once be estimated from its chemical analysis, without the possibility of any error except those arising from errors in the analysis; and estimates so made are much more trustworthy than any experiments with steam boilers or with water-calorimeters of any kind.

5. *Application to Rock-oil.*—According to a Paper by Professor H. D. Rogers, LL.D., lately read to the Philosophical Society of Glasgow, rock-oil consists of a mixture of various hydrocarbons, whose composition ranges from 18 equivalents of carbon with 20 of hydrogen, to 26 equivalents of carbon with 28 of hydrogen. The weight of the equivalent of carbon being six times that of the equivalent of hydrogen, the composition of the ingredients of rock-oil by weight is found to range from 27 of carbon with 5 of hydrogen in 32 parts by weight, to 39 of carbon with 7 of hydrogen in 46 parts by weight. Hence the following estimate of the greatest possible evaporative powers of those ingredients:—

	Evap. powers of Constituents.	Most Hydrogen. Parts by Weight.	Most Hydrogen. Products.	Least Hydrogen. Parts by Weight.	Least Hydrogen. Products.
Carbon, ...	15	27	405	39	585
Hydrogen, ...	64	5	320	7	448
Sums, ...		32	)725	46	)1033
Greatest possible evaporative powers of Compounds, ...			22·7		22·5
Ratios to the evaporative power of Carbon, ...			1·51		1·5

Hence it appears that the greatest possible evaporative powers of the ingredients of petroleum differ but little in any instance from 22½, being *once-and-half the total evaporative power of carbon;* and such will also be nearly the ratio of the available evaporative powers, when each sort of fuel is burned in a furnace and boiler specially adapted to it, so as to give the same *efficiency,* or ratio of available to total heat.

MINUTES.

Anderson's University Buildings, November 1, 1865.

THE Sixty-fourth Session of the Philosophical Society of Glasgow was opened this evening—DR. ALLEN THOMSON, Vice-President, in the Chair, in absence of Professor Rogers, the President.

The Society confirmed the following minute of a meeting of Council, held on the 29th of July:—"It was agreed to insert the words "more than" in Rule X., before the words "four-fifths of the votes," so as to make the second sentence in the Rule read as follows:—"The person proposed shall be balloted for at the next regular meeting, and he shall be elected if more than four-fifths of the votes tendered are favourable." This correction is necessary to bring Rule X. into conformity with Rule XIV.

DR. ALLEN THOMSON delivered an opening address, in which, after offering some suggestions for increasing the interest of the proceedings of the Society, he directed attention to the extensive scheme of city improvement contemplated by the Municipal authorities, and pointed out how it might be made available for sanitary reform. He also referred to the necessity of its being accompanied by an adequate measure for the disposal of the sewage of the city.

Dr. THOMAS ANDERSON also made some observations on the subject of sewage, and was followed by Mr. James R. Napier and Mr. Rowan.

A cordial vote of thanks was given to Dr. Thomson for his address.

November 15, 1865.—DR. ALLEN THOMSON, *Vice-President, in the Chair.*

The annual meeting of the Society for the election of Office-bearers and for other business was held this evening.

The following gentlemen were elected members of the Society, viz.:—

Mr. William West Watson, city chamberlain, Glasgow.
Mr. James Robertson, engineer, 16 Arlington Street.
Mr. William Wingate, Jun., merchant, 14 Lynedoch Crescent.
Mr. William Lang, Jun., chemist, Crosspark, Partick.
Alexander T. Machattie, Ph.D., F.C.S., lecturer on chemistry, and analytical chemist.

The SECRETARY read the following Report from the Council on the state of the Society:—

REPORT BY THE COUNCIL ON THE STATE OF THE SOCIETY.

I.—*Proceedings of last Session.*—The printed *Proceedings* of the Society have always formed a small proportion of the papers read in any one session; and in the last, the proportion was less than usual, although the subjects brought forward and discussed were of the average variety and interest, as appears from the following classification of them:—Three of the papers were on Glacial Phenomena—one by Mr. John D. Campbell; one by Dr. Bryce and the Rev. Mr. Crosskey, conjointly, on the supposed Occurrence of Fossils in the Boulder Clay (Dr. Bryce's account of the Arran and Kilmaurs beds, and Mr. Crosskey's of the Chapelhall beds, have been since published in an extended form in the *Journal of the Geological Society of London*); a third paper, the joint communication of the Rev. Mr. Crosskey and Mr. David Robertson, being on new additions to the Fauna of the Glacial epoch, was not sufficiently matured to be printed, but will appear in a subsequent number of the *Proceedings*, with illustrations. The fundamental laws of Optics were illustrated, with the aid of a set of mechanical models, by Mr. Foster, who, since last session, has received a scientific appointment in London; and the same gentleman delivered a lecture on Spectrum Analysis, illustrated by a fine series of experiments. Professor William Thomson favoured the Society with two communications, one a description of various new instruments, of his own invention, for remedying the defects of the ordinary Electrometer; the other, on Secular Variation of Terrestrial and Atmospheric Temperature. The various plans employed for utilizing the sewage of great cities were explained by Dr. Thomas Anderson. Professor Grant described the different methods which have been proposed for determining the distance of the Sun from the Earth. Two of the papers printed in the *Proceedings* were contributed by Dr. John Taylor, one on a Means of Preventing Accidents in Diving Bells; the other, descriptive of an Electric Apparatus for Detecting Pieces of Metal in Gunshot Wounds. The only other paper inserted in the *Proceedings* was by Mr. William Gorman, on a Heat-restoring Gas Furnace for Heating Iron. These, together with several other topics introduced incidentally, without the formality of written communications, completed the ordinary proceedings of the session; to which the Council added the revised Rules of the Society, afterwards printed and distributed amongst the members.

II.—*Place of Meeting.*—At the opening of another session the Council has had its attention anew directed to the inconvenience of the Society's present place of meeting. The residences of a great majority

of the members, as may be seen in the list, are at a considerable distance from this Hall, and very generally situated in the western parts of the city. This has the effect not only of preventing a larger attendance, such as the interest of the proceedings might frequently be expected to attract to the meetings, but of actually occasioning the resignation of a notable proportion of the members every year; while the same cause may be presumed to discourage an equal number from entering. The imperfect ventilation of the Hall is also a source of continued complaint by members attending the meetings. The means resorted to a few years since to prevent the access of moisture to the books in the Library have only proved partially successful, and many of the books are becoming injured by damp. It should not be overlooked, also, that the Hall is situated in dangerous proximity to an extensive oil establishment, which is itself contiguous to a wright's shop, and that the risk of the Library's being injured or destroyed by fire is thus not inconsiderable. In these circumstances, the Council is of opinion that the time has come when the Society should take measures for obtaining suitable premises in a more convenient district of the city, and which might be fitted up so as not only to be adapted for the accommodation of the Library and the meetings of the Society, but also for the occasional resort of the members during the day. There is reason to expect that the increased expenditure occasioned by removing to the westward would be counterbalanced by fresh accessions to the ranks of the Society.

III.—*Number of Members.*—After having hovered for several years on the borders of 300 members, the Society has gradually diminished to 279, of whom 14 are at present in arrears of their annual subscriptions for two years. Members in these circumstances have hitherto been dropped from the list at the end of the third year; but under the revised Rules, no member is allowed to withdraw from the Society until he shall have paid his arrears, and given in a written notice of his resignation.

IV.—*Lectures.*—The revised Constitution of the Society provides for the occasional delivery of Lectures on topics of scientific interest, which shall be open to the public, under such regulations as the Council may consider expedient. The Council hopes soon to be able to submit a proposal to the Society on this subject.

V.—*The Library.*—The state of the Library will be reported upon by the Librarian. In its management the Council is strongly of opinion that the money voted for the purchase of books ought to be expended on the acquirement of standard works in the various departments of science, and especially those which are too costly to be

within the reach of private individuals. The Council will be glad to receive suggestions from the members as to the choice of books of this description, in the various sciences with which they are conversant; and in order to give a definite character to their co-operation, it is proposed to revive the Sections of the Society, with the view of each appointing a representative to act along with the Council.

VI.—*Discussions.*—As much valuable information is sometimes elicited in the discussions on the papers read in the Society, the Council proposes that an outline of the discussions should be printed in the *Proceedings*, along with the papers, subject to the discretion of the President and Secretary, and that a trial be made of the plan during this session. The Council has agreed that for this purpose the services of a professional Reporter be obtained on special occasions.

VII.—*The Exhibition Fund.*—The money accruing from the Exhibition of 1846, with interest, now amounts to £976. The conditions on which the money was lodged with the City Corporation will be found in the minutes appended to the printed *Proceedings* of last session. The Council sees considerable difficulties in the way of obtaining possession of the money for the use of the Society, and continues to give its attention to the subject. The Council will be glad to receive any suggestions from members of the Society, which may be serviceable in directing their procedure in this matter.

The adoption of the Report was moved by Mr. Alexander Harvey, seconded by Mr. James R. Napier, and agreed to.

Dr. Bryce, the Librarian, gave in a report on the Library, which now contains 3,739 volumes.

The adoption of the Report was moved by Dr. Francis H. Thomson, seconded by Mr. George Smith, and agreed to.

Mr. James Reid, the Treasurer, gave in the following Abstract of the Treasurer's Account for Session 1864-65:—

Dr.

1864.—*Nov.* 1.

To Cash in Union Bank of Scotland,	£28	19	0			
,, Cash in Treasurer's hands,	11	10	10½			
				£40	9	10½
,, Entry-money and dues from 11 new Members at 42s.,	£23	2	0			
,, Annual Dues from 5 Original Members, at 5s.,	1	5	0			
,, Do., from 1 Original Member for two years,	0	10	0			
,, Do., from 245 Members, at 21s.,	257	5	0			
,, Do., from 6 Members for two years,	12	12	0			
				294	14	0
,, Institution of Engineers for Rent,				15	0	0
,, Interest on Bank Account,				2	0	2
,, Taxes recovered from Landlord,				2	13	3
				£354	17	3½

CR.

1864.—*Oct.* 31.

By New Books and Binding,			£90 14 5
,, Printing Proceedings, Rules, Circulars, &c.,			35 17 5
,, Stationery,			6 4 3
,, Subscription to Ray Society (two years),			2 2 0
,, Salaries, Wages, Delivery of Circulars, &c.,			117 7 4
,, Rent, Insurance, Gas, &c.,			53 5 8
,, Petty Charges, Postages, and Taxes,			7 13 2
,, Balance—In Union Bank of Scotland,	£40 11 2		
,, Treasurer's hands,	1 1 10½		
			41 13 0½
			£354 17 3½

The Society then proceeded to the Sixty-fourth Election of Office-bearers.

DR. ALLEN THOMSON said that he was sure the entire Society sympathized with Professor Rogers, the retiring President, in the condition of infirm health which had prevented him from attending the Society so regularly as he had wished, and as the members desired for their own benefit. He hoped that some of the circumstances which had weighed on their friend's mind were now at an end; that the removal of causes of irritation in his native country would tend to the renovation of his health; and that Dr. Rogers would speedily be restored to us with a revival of those remarkable powers of philosophical acumen and eloquent enunciation of scientific principles which had won for him a position of the highest eminence in this the country of his adoption. The Vice-President then proposed that Dr. Francis H. Thomson be elected President of the Society.

The motion was seconded by MR. HART, and unanimously carried.

The rest of the Office-bearers were appointed as follows:—

President.

DR. FRANCIS HAY THOMSON.

Vice-Presidents.

PROFESSOR ALLEN THOMSON, M.D., F.R.S.

JAMES BRYCE, LL.D., F.G.S.

Librarian.

REV. HENRY WILLIAM CROSSKEY.

Treasurer.

MR. JAMES REID.

Secretary.

MR. WILLIAM KEDDIE.

Other Members of Council.

Mr. James Napier.	Mr. Edmund Hunt.
Mr. Robert Hart.	Mr. William Neilson.
Dr. John Taylor.	Mr. James King.
Mr. James R. Napier.	Mr. Daniel Macnee.
Prof. W. T. Gairdner, M.D.	Mr. John Burnet.
Prof. Robert Grant, LL.D.	Prof. Thomas Anderson, M.D.

Dr. Allen Thomson exhibited and described the skull of a Gorilla, an adult male. He directed attention to the striking difference in shape between the skull of the male and that of the female; also between the young and the adult form; to the enormous development of crests of bone in the head for the attachment of muscles; to the nature of the dentition and the large development of the canine and wisdom teeth; to the situation of the occipital condyles, showing the nearly erect position of the animal; to the rectangular line of the jaw; to the weight of the brain, being about fifteen ounces; to the points of resemblance between this and the human skull, and the much more important points of distinction between them.

November 29, 1865.—Dr. Francis Hay Thomson, on taking the chair, acknowledged his sense of the honour which had been conferred upon him by his election to the office of President. He adverted to various methods of increasing the interest of the Society's proceedings and enlarging its influence; and he expressed his earnest desire to justify the confidence which the Society had reposed in him, by devoting his most strenuous efforts to promote its prosperity.

Mr. Thomas Small, 15 Berkeley Terrace; Mr. William Duncan, Coltness Iron Company; and Mr. Alexander Donaldson, iron merchant, 103 St. Vincent Street, were elected members of the Society.

The President brought under the notice of the Society a communication from Mr. William Ferguson, stating that the Liverpool Chamber of Commerce had originated a public subscription for a Testimonial in acknowledgment of Admiral Fitzroy's services to the science of practical Meteorology, and in furtherance of which an influential committee of noblemen and gentlemen had been appointed. Mr. Ferguson was of opinion that the proposal could not be more appropriately made known to the citizens of Glasgow than through the Philosophical Society, of which he was formerly a member. The President added

that the Council would bring before the next meeting of the Society a definite proposal on the subject.

The following papers were read, viz.:—

"On the most Profitable Speed for a fully-laden Cargo Steamer for a given Voyage." By Mr. James R. Napier.

"On the Luminosity of the Sea." By Messrs. David Robertson and William Keddie.

December 13, 1865.—*The* President *in the Chair.*

Mr. George William Munro, M.A., merchant, 4 Melrose Street; Mr. William Holms, manufacturer, 9 Park Circus; and Mr. J. B. Gilmour, engraver, 3 Royal Exchange Place, were elected members of the Society.

The President read a statement which had been approved of by the Council, recommending to the Mercantile and Shipping interest in Glasgow the Testimonial in acknowledgment of the late Admiral Fitzroy's services to the science of practical Meteorology, and especially to the mercantile marine of the country, by forecasting storms on principles which would be applied by his successors. It was agreed that the statement should be laid down at the bar of the Royal Exchange and of the Western Club.

The President intimated that James M'Lagan having resigned the office of Sub-Librarian, the Council had appointed John M'Quaker in his place.

Mr. James R. Napier read the remainder of his paper "On the most Profitable Speed for a fully-laden Cargo Steamer for a given Voyage."

There was read a communication from Professor Henry D. Rogers, dated "Boston, Massachusetts, United States, 16th November, 1865," "On Petroleum,"—its Nature, Distribution in Depth, Durability of Supply, Classification of Mineral Oils, their Geological Relations, Individual Peculiarities of some Oil Wells, "Life-time" of Oil Wells, Surface Indications of Subterranean Petroleum, Statistics of Annual Produce.

On Wednesday, the 10*th of January*, Mr. John Zephaniah Bell, of London, delivered a Lecture, under the auspices of the Society, in the Corporation Galleries, "On Story in Fine Art."

On Tuesday, the 16*th of January*, Mr. Bell delivered a second Lecture in the same place, the subject being "Story in Ornament."

January 24, 1866.—*The* PRESIDENT *in the Chair.*

Mr. R. Bruce Bell, Civil Engineer, Clifton Street, and Mr. James M'Intosh, 3 Abbotsford Place, were elected members of the Society.

Dr. Bryce presented four volumes of the "Diplomatic Correspondence of the United States, in 1864," the gift to the Library of J. M. Bailey, Esq., M.D., United States Consul in Glasgow. The thanks of the Society were voted to Dr. Bailey, to be communicated by the Secretary.

Mr. James R. Napier presented a pamphlet, 4to, "On the Magnetic Character of the Armour-plated Ships of the Royal Navy," by Frederick John Evans, Esq., Staff-Commander, Royal Navy. The thanks of the Society were voted to Commander Evans, to be communicated by the Secretary.

PROFESSOR WILLIAM THOMSON exhibited and explained an improved arrangement of Magnets for the Electro-magnets and Coil used in the Jones' system of controlling Clocks electrically. He also exhibited and described several new Electrometers, constructed under his direction by Mr. Whyte, instrument-maker, Glasgow.

February 7, 1866.—*The* PRESIDENT *in the Chair.*

The following gentlemen were elected members of the Society, viz.:—

Mr. George Birrell, merchant, 62 Jamaica Street.
Mr. Matthew Muir, Tradeston Mills.
Mr. St. John Vincent Day, civil engineer, 166 Buchanan Street.
Mr. James Bell, 261 West George Street.

The PRESIDENT stated that the Council had agreed to request the members of the Society to intimate to the Secretary to which of the Sections of the Society they wish to attach themselves, conformably to Rule IX.; and that a similar request be inserted in the circular calling the next general meeting.

MR. RICHARD BROWN read a paper, entitled, "A few Remarks on the Treatment of the Cattle Plague."

February 21, 1866.—*The* PRESIDENT *in the Chair.*

Mr. Gregory Bird, tar and oil distiller, 8 Berkeley Terrace, was elected a member of the Society.

The following members were appointed to represent the Society in a General Committee of the Philosophical, Scientific, and Art Societies of Glasgow, to consider a proposal for accommodating these institutions in one building, viz.:—Dr. Francis Hay Thomson, President, the Lord Provost, Dr. Allen Thomson, Dr. James Bryce, Professor William Thomson, Mr. William West Watson, city chamberlain, Mr. Walter Crum, Mr. James Reid, Mr. Alexander Harvey, Mr. Michael Connal,

Mr. George Anderson, Mr. John Burnet, architect, Mr. R. Bruce Bell, Mr. James King, Mr. James Napier, chemist, Mr. William Ramsay, Mr. Robert Hart, Rev. Henry W. Crosskey, Mr. Daniel Macnee, Mr. James Young, Mr. Neil Robson, Mr. William Keddie.

PROFESSOR GRANT read a paper "On the Determination of the Longitude of the Glasgow Observatory by Galvanic Signals."

March 7, 1866.—*The* PRESIDENT *in the Chair.*

Mr. Peter Ferguson, brush-maker, 296 Bath Crescent, and Mr. John E. Poynter, 72 Great Clyde Street, were elected members of the Society.

A letter was received from the Secretary to the Imperial Geographical Society of Vienna, inviting an exchange of publications between the two Societies. The Secretary was instructed to intimate compliance with the request, and to forward a complete series of the *Proceedings* to the Imperial Geographical Society of Vienna.

MR. CROSSKEY, the Librarian, reported the presentation to the Library of a series of publications from the Royal University of Sweden, including memoirs on Natural History by Sars, Hoch, and Siebke, and astronomical observations by Astrand; also, a part of the *Transactions of the Imperial Academy of Sciences of St. Petersburg*, The Librarian was authorized to send copies of the *Proceedings* of the Society to the different Continental Universities, should they agree to grant their Transactions in exchange.

The PRESIDENT called attention to the notice in the billet, stating that the Council recommended to the members to attach themselves to Sections of the Society according to the objects of their pursuit.

A letter was received from Mr. James R. Napier, resigning his office as a member of Council. The Society agreed, on the recommendation of the Council, to request Mr. Napier to retain his office till the close of the present year.

PROFESSOR GAIRDNER read a paper "On the Function of Articulate Speech, and on its connection with the Mind and the Bodily Organs; illustrated by a reference to recent Observations on certain Diseased States of the Brain."

DR. SANDERS, of Edinburgh, supplemented the paper with some observations; and in the discussion which followed, Dr. Allen Thomson, Mr. Crosskey, and the President, took part.

March 21, 1866.—DR. BRYCE, *Vice-President, in the Chair.*

MR. CROSSKEY reported the following gifts to the Library:—

On the Expansion of Saturated Vapours. 8vo, pp. 8. By Professor W. J. Macquorn Rankine. From the Author. Thanks voted.

On the Secular Change of Temperature of the Air at Greenwich. 8vo, pp. 8. *On the Height and Velocities of the November Meteors of* 1865. 8vo, pp. 6. By Mr. A. S. Herschel. From the Author. Thanks voted.

Dr. Bryce made a communication "On the Geological Structure of the Earthquake District of Perthshire."

Mr. W. Keddie read an account of recent observations by M. Milne Edwards, M. Pasteur, &c., on the subject of so-called Spontaneous Generation.

April 4, 1866.—*The* President *in the Chair.*

The President intimated that the Council continued to give its careful attention to the obtaining of suitable accommodation for the Society, but had no immediate prospect of a successful issue to its inquiries.

Dr. Thomas Anderson gave an account of "Some Recent Changes in the views entertained by Chemists regarding the Constitution of Chemical Compounds."

April 18, 1866.—Dr. Allen Thomson, *Vice-President, in the Chair.*

The following gentlemen were proposed as members of the Society, viz.:—Mr. Paul Dorn, chemist, 151 North Street, and Mr. Sigismund Schuman.

Mr. Edward A. Wünsch gave an account of recent discoveries made by him in the Coal Formation in Arran.

Mr. Young, of the Hunterian Museum, made some supplementary observations.

Dr. Allen Thomson exhibited and described the Hearts of the Reptilia.

Professor W. J. Macquorn Rankine gave in a paper "On the Estimation of the Evaporative Power of Fuel."

The Vice-President, in announcing the termination of the Session, congratulated the Society on the increase of its members and the variety and interest of its proceedings during the past winter, and hoped that the members would all return at the commencement of another session, resolved to carry on the business of the Society with unabated vigour.

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

SIXTY-FIFTH SESSION.

I.—*The Workshops of Glasgow.*

By the PRESIDENT, DR. FRANCIS H. THOMSON.

Read November 7, 1866.

GENTLEMEN,—Allow me to thank you for your presence here this evening, and congratulate you upon our once more meeting, on this the Sixty-fifth Session of our time-honoured Society. During the long period of its existence it has gradually increased the number of its members, and truly shown the important results of its proceedings. In a great city like Glasgow, where the arts and sciences have kept pace with its population and prosperity, it is doubly necessary that a Society like this should not only be fostered by all well-thinking men, but take a high place amongst the educational institutes of this constantly and rapidly increasing city. Our library is valuable and large, and now contains 3,875 volumes, amongst which may be found all the best books for reference in every specialty, and it is the anxious desire and study of the Council to increase such facilities. Before going into the subject-matter of my address to you this evening, allow me briefly to refer to the serious loss that the Society has sustained in the death of Professor Rogers, our late much-respected President,—a man who, amongst his many high qualities, combined deep research with great power of communicating his valuable knowledge. And I feel that we must all deeply regret that such a man has been taken from us in the prime of his life, and when he was becoming so eminently useful in his own special department of science. During the last session of his presidentship, in consequence of the feeble state of his health, he was not much amongst us, and this I know he regretted. It was pretty evident to all who had the pleasure of his acquaintance, that his health had been seriously shaken, but no one could realise that the end was so near. He went to America for the purpose of bracing himself up for the

winter campaign, but only returned to be attacked by his last and fatal illness. It may well be said of him, that "take him for all in all, we shall not look upon his like again."

So many eminent men have from year to year addressed you from this chair, and given so much containing deep and scientific matter, that I may well feel diffident in placing before you the observations which I have prepared. In thinking over what I had to say to you this evening, it appeared to me that, former Presidents having generally given a sort of condensed report of what was extraordinary or new in science during the past year, it struck me that I might, with justice to you and myself, give you a comparative vidimus of the rise and progress of some of the workshops of Glasgow; and although it would be impossible in any address like this to go into the details of each and every trade, I think a judicious selection may be made of those which have tended most to our prosperity; and as iron forms a most prominent element, I propose first to take up this subject.

If we look back to the past history of Glasgow, and compare the population 80 years ago with what it is now, being a difference between 60,000 and 440,000 in the census in 1865, one cannot help being very much astonished at the almost unprecedented increase both of our population and commercial prosperity. At that early date there were no shipbuilders, power-loom factories, ironfounders, machine-makers, or iron-masters. There was one engineer and a coal-master. In the Directory for the year 1866 we find—

194 Engineers.	13 Iron Fence Makers.
138 Coal-masters.	16 Lock and Hinge Makers.
84 Iron-masters.	8 File Cutters.
124 Founders.	8 Metal Cast Makers.
160 Iron Merchants.	31 Wire Rope Makers.
119 Ironmongers.	44 Tube-makers.
34 Edge-tool Makers.	7 Iron Bedstead Makers.
77 Machine-makers.	13 Gun-makers.
23 Nail-makers.	2 Large Locomotive Engine Works.
16 Chain-makers.	

as also sundry other minor divisions; and, to sum up the whole, we have nineteen large shipbuilding yards, turning out, during the year 1865, iron steamers, iron sailing ships, barges, composite steamers, sailing ships—242 in all, with a tonnage of 153,032.

In the year 1864 the total steam shipping vessels registered for all England was 2,490, with a tonnage of 679,281. Glasgow at this date owned of this number 233, being about one-tenth in quantity, and one-eighth in tonnage of the whole mercantile steam navy of Great Britain

and Ireland. As an indication of the increased demand for iron vessels, in 1865, out of the gross number, 242, there was not one wooden steamer. And to carry out this object 25,000 men were employed, which on a fair average would represent 100,000 souls—three individuals being apportioned to each workman; and £4,000,000 being required to bring about this result. And in quoting the prosperity and practical knowledge of the shipbuilders on the Clyde, it is matter of much gratification for us to know, that in the great China tea race which took place this last year, three out of the four were built at Greenock—viz., the "Taeping," 767 tons; the "Ariel" 853 tons; and the "Serica," 708 tons,—the "Taeping" arriving first, and the others within an hour after. One great element of prosperity observable in this special department of applied science is of course very much, if not altogether, the result of our productive coal and iron fields, and we cannot help being very much struck at the rapid elimination of these industrial results.

As at the 29th of December, 1865, we have in Scotland—and principally connected with Glasgow—twenty great proprietors of iron smelting works, represented by thirty establishments containing 163 furnaces; and at this date 163 were in blast and 29 out of blast,—the quantity of iron produced being 1,164,000 tons, requiring for its production—

Coal, . 19,100,000 tons; | Dross, . 7,566,000 tons;
Cal. Bl. Band, . . 19,788,000 tons;

and, taking the cost for wages alone at 36*s*. per ton, the total capital represented would be £2,095,200. At the same date the shipments from Scotland were 1,272,000 tons, and the stock in hand 652,000 tons. One might go on to any extent in giving statistics of these great elements, iron and coal, as being the active agents in this rapid increase of commercial Glasgow, but sufficient has been said to show the importance of the subject.

In endeavouring to place before you the history of some of the most important workshops which have conduced to this prosperous state of matters, I shall endeavour to explain, as practically as possible, the result of each work which I visited; and, in the first place, allow me to carry you along with me to the large and important establishments of Messrs. Randolph & Elder, who are not only very extensive engine makers, but possess one of the most important building-yards on the Clyde.

On first entering the engine works, the magnitude of the building is at once apparent, the length being 270 feet by 100 broad, exclusive of the galleries, which, in fact, are large and deep workshops. The whole

building is covered in with a glass roof, giving ample light and protection to all the departments. On looking up you see gallery rising above gallery, where from 500 to 600 men are all earnestly employed at turning, slotting, planing, boring, and pattern-making; in fact, engaged in the production of all the different materials required in their large and expensive business. The first important object that strikes the eye on entering the yard is a compact beam-engine, working up to 80 horse-power, which drives the whole machinery in the place. The first department visited was the engine-fitting yard. This consists of a space 200 yards long by 50 broad, forming the back of the work, fitted up with four steam derrick cranes, each capable of lifting five tons, which can be combined and concentrated almost at any point. Immediately below the roof there is a large travelling crane capable of lifting sixty tons. The wheels of this crane fit on the outside of the rails, and thus tend to increase the strength and stability of the 200 feet wall, which otherwise might some day bulge out under the influence of a heavy pair of engines. The strength is also increased by heavy iron stays on the inner side. Beyond this rail and towards the other wall, you have a reflection of the galleries coming round from the main-yard, under which may be observed a number of large slotting and turning machines, applicable to heavy work, both vertical and horizontal. At the period of my visit the yard was rather empty of work, several heavy pairs of engines just having been sent out, but there were two pairs pretty nearly finished—one pair, 80 horse-power, for Spain, and the other, 160 horse-power, for China.

We now proceed to the blacksmith shop, where 30 smiths' fires and three steam-hammers (one by Condy, and the two others by Rigby & Beardmore) were all hard at work, forging, in the rough, all the different parts required in the production of an engine. Next to this was the large store-room, containing stock of all kinds, checked by clerks, who register every item given out. Up stairs, in the first gallery, which surrounds three sides of the work, are somewhere like 80 lathes, punching and slotting machines of a smaller description than those in the basement, with about the same number of vices. Above this, in gallery No. 2, pattern-making of every description was going on; also the adaptation of wooden teeth to iron wheels, which is much preferred for certain kinds of work. A portion of this gallery is devoted to the manufacture of donkey engines, of which 250 are generally produced yearly. The third gallery was principally used as a drawing-room and certain portions of fine pattern-making. The fourth gallery was partly used for the different sawing machines, and amongst them the endless band saw. This beautifully-arranged work has turned out

during the last twenty-seven months 5,565 horse-power, besides doing a very large general business.

I afterwards paid a visit to the large building-yard, which covers 17 acres of ground, and is fitted up with every requisite for carrying out their extensive business; but I cannot do better than here quote a letter which Mr. Elder was kind enough to write me, in answer to one from myself, requesting permission to see the works, and I should herewith wish to acknowledge the extreme courtesy with which I was received by both the partners, Mr. Randolph himself accompanying me over the works:—

"Glasgow, 13*th Sept.*, 1866.

"F. Thomson, Esq.

"My dear Sir,—Referring to your favour of yesterday, I need not say it will give my firm much pleasure to see you at any time, for any purpose, in any of the works, and that if I can be of any assistance in giving you statistics for your proposed paper, I shall do so with much pleasure. Our trade is a very variable one, but the last three years should form something like what we could do when fully occupied. There have been large additions made since last year, which have not yet been brought into operation, and we are about to make further extensions, which will also add considerably. Our present capacity is about a vessel of 1,000 tons every two weeks, fitted with engines complete. We have not done so much as this quantity, however, and I suppose the best way is to state the amount in money, say about £800,000, during the last two years. The wages bill is upwards of £200,000 for that time, and the number of men (including labourers and apprentice boys) has varied from 3,200 to 2,000; though we expect to do a half more work with a half more men, should circumstances require.

"We launched in the last twenty-seven months—

5	Paddle Steamers,	850	tons each.	4,250	10,000
5	Do.,	2,000	"	10,000	2,000
6	Screw Steamers,	750	"	4,500	480
2	Do.,	1,300	"	2,600	550
2	Do.,	850	"	1,700	200
2	Do.,	1,200	"	2,400	400
1	Do.,	1,500	"	1,500	350
4	Do.,	150	"	600	60
1	Do.,	500	"	80	—
"Composite Steam, Teak Plank and Iron Frame—					
2	Paddle Steamers,	1,050	"	2,100	600
1	Screw Steamer,	1,050	"	1,050	150
1	Do.,	850	"	850	125
32				31,630	5,655

"We have also just completed two Floating Graving Docks, iron, viz.—one in Saijon Cochin China, for the French Government, and the other in Callao, in Peru, for the Callao Dock Company. These Graving Docks have each about 3,000 tons of malleable iron, and are fitted up with powerful pumping engines, capable of lifting the largest class man-of-war out of the water in less than three hours.

"Each dock is 300 feet long, 76 feet broad, and 40 feet deep in the walls, and capable of docking ships drawing 27 feet of water. These docks taken together may equal 2,000 tons of ships; they cost complete £100,000 each. We also make a good deal of gearing for cotton mills and factories of every description.—Yours truly,

(Signed) "JOHN ELDER."

PARKHEAD FORGE—RIGBY & BEARDMORE.

I had promised myself much pleasure in paying a visit to the above mentioned works, and I assure you, gentlemen, it was fully realized. I went out quite unexpectedly, and was met by both the Messrs. Rigby, who represent the large interests of this most important establishment. I came upon them at a time when they were but just recovering from the effects of a fearful accident, which must be fresh in the memories of all,—viz., the sudden explosion of one of their vertical steam boilers, by which seven men were killed and many wounded; and although I could not discover any trace of hiatus that had been made, yet one could not help being struck with the sad appearance of the whole place. Of course, the work looked like a bee-hive, and all the men were evidently intent upon their various duties; and when one contrasted the blazing furnaces in active employment in various parts of the work with the desolate darkness of that portion where the accident had taken place, a melancholy feeling stole upon me which was very difficult to avoid in going over the practical details, which were so well brought before me and explained by both gentlemen.

The first place we entered was the machine shop—a well-constructed building of about 150 feet in length, and about 50 broad. This contained several huge instruments, which, compared with what I had seen in other works, looked like huge leviathans. In the first place, there was an engine of 15 horse-power entirely devoted to this place, which had for its work the driving of the various lathes and slotting machines, capable of executing work for the largest vessels in the world. For instance, I saw in process of planing, by one of the slotting machines, a rudder frame for a transport vessel, weighing nine tons. The turning-lathe is also one of the largest in the world; and they have also a boring machine, capable of going through large masses of iron, the diameter of the drill being 10 inches. The large planing

machine was made by Harvey, and is the largest in Scotland, except the Messrs. Napiers', as also the slotting machine made by Shanks. There were also various other turning machines, in which I observed large double crank-shafts, all in active operation.

The smithy was the next shop we entered, and a most extraordinary appearance it presented, as the time of my visit was after dark; and to see the men running about like so many fiends, gave the place a most extraordinary appearance. Although difficult to define the multifarious operations carried on in this shop, yet the most apparent I shall attempt to describe:—To work the huge masses of metal you have cranes of the most powerful description, acting in concert with the masses of iron which are brought out of the heating furnaces: and to show the power at their command, I may mention that they have them in the following order:—

4 capable of lifting, ...	40 tons.
2 "	50 "
4 "	12 "

These supply food for thirteen steam hammers, principally of the patents of Rigby and Naysmith, the average weight of the blocks being from one to seven tons.

The next, the scrap-shop, is a very important apartment, and well worthy of notice. On first entering, you are surrounded by perhaps 400 or 500 tons of scrap iron of all descriptions, which is being conveyed by boys to a pair of large double-acting shears, capable of cutting through 3 inches of solid iron as you would cut a piece of cheese, and whose huge maw is constantly supplied by ten corps of twenty or thirty boys, superintended by one steady foreman. If a piece of cast iron unhappily gets amongst this, it flies off, and might injure a number of them. An instance of this took place whilst I was looking on, and the poor unfortunate boy who brought the piece got into great disgrace, as it flew about in every direction, to the personal danger of all concerned. From this the iron is taken, weighed, and placed in parcels, and from thence conveyed into the shingling house; where it is first put into what you would call the scrap furnaces, then into the heating furnaces; from thence it is taken and placed under the steam hammer, where it is shaped and brought into a condition to be welded and formed into all the various shafts and cranks for which the work is famous. Each steam hammer does the work of four furnaces. As a result of this shop, I saw in another part of the work an immense crank applicable to 200 horse-power; and Mr. Beardmore told me that they had that day got an order from Government for one weighing 36 tons; that of the "Black Prince," which was made by them for the Messrs. Napier, only weighing 27

tons. This is the more extraordinary as to bulk of material, for one-half of that used is lost, in the shape of silicate of iron, during the different processes to which it is subjected in its progress through the works.

The rolling mills next attracted my attention; and although not strictly devoted to rolling, seeing that at one end you have thirty puddling furnaces, seven re-heating furnaces, and two shingling machines, yet the one is so intimately connected with the other as to be economically placed under one roof, which covers a space 300 feet in length by 150 feet wide. The rollers are driven by two horizontal engines of 300 horse-power, made by Messrs. Napier & Sons, of the Lancefield Works, supplied by fourteen vertical boilers, heated from the puddling furnaces and others, and driving a fly-wheel of 18 tons weight, at 100 revolutions per minute. What might be the effect of an accident in this shop you may well conceive.

The iron produced from this place is principally boiler-plate, and formerly armour-plates for vessels of war. But such has been the competition lately in England, that for some months past this portion of the work has been silent; but sufficient still goes on to absorb the greater portion of their powerful engines. The iron from which the boiler-plate is produced is puddled iron, made principally from Messrs. Baird & Co.'s best brand, in which the aforementioned puddling and reheating furnaces are used. The men in this shop are all on piece-work; and many of them can make, if so disposed, £1 per day. But, alas! I am told that those men are constantly in degrading poverty. Whilst the proprietors of this work were tempted into the manufacture of armour-plates, they gained a very high character; and if they had the convenience of a railway into their works, would, I have no doubt, compete with the best houses in England; but the expense of carriage is so immense—some of those plates manufactured by them weighing as much as 12 tons each—they have thought it wise at present to avoid competition.

The annual quantity of coal used in this work is 60,000 tons, and the quantity of iron consumed is 15,000 tons. The ordinary number of men is 700, and the average fortnight's wages £1,400; when in full work, as much as £1,700.

ATLAS WORKS—J. M. ROWAN & CO.

Having thus given you a somewhat detailed account of some of the large engineering works, I come to one which possesses much interest—namely, that of our friend and member, Mr. John M. Rowan, of the

Atlas Works, who has shown great energy and determination of character in the adaptation of anything new in engineering or applied science, and whose latest endeavour has been the practical application of Mr. Bessemer's process for making steel. I do not intend to give you a laborious description of all the early struggles which were met with in bringing about this simple and beautiful process—for on many occasions in this room it has been well described, and may be partially known to all—but it will be right to refresh our memories with the general features of what is likely to turn out one of the greatest discoveries of the day. Mr. Bessemer, whilst investigating the possibility of producing a malleable metal in a cheap form, suited to the manufacture of ordnance, tried a mixture of cast steel and cast iron, without any great success; he at last hit upon the idea, that by blowing common air through melted iron, a certain purification would take place. A quantity of cast iron having been melted in a cupola furnace, it was run into an iron vessel lined with fire-clay, which had previously been brought to a white heat by the use of a blast engine, and this air being forced through and amongst the melted metal, produced such a dazzling, volcanic irruption of brilliant coruscations as appeared to be almost blinding.

The result of this operation was a steel of good quality. Of course, this new process produced intense excitement amongst metallurgists of the day, and although to a certain extent imperfect as to the practical working properties of the metal produced, it formed the nucleus of what is now one of the most perfect processes that we know in metallurgy. To Messrs Rowan & Co. belong the credit of practically introducing this process into Scotland; and although they admit at their first outset great difficulties in the practical and economical use of the process, they are now, after two years' hard work, beginning to reap the benefit of their spirited enterprise. The process, as carried out by them, may be thus described; and this information was furnished to me by Mr. Rowan, jun., who takes full charge of this department in the works:—

The process is carried on in a fire-proof house, and, on entering, the most conspicuous object is unquestionably the convertor, a vessel of a pear shape, made of stout boiler-plate, and lined with a powdered silicious stone known as gannister—a far more durable substance than fire-brick. The convertor is mounted on trunions, which rest on stout iron standards, and it may be turned in any required position by means of gearing worked by hydraulic power. There is an opening at the top for filling it with the molten crude iron, and for pouring out the steel after being manufactured. At the bottom of the vessel are inserted seven fire-plate tuyeres, each having seven holes, through which the

blast of the engine is admitted. Before commencing to charge the convertor, its interior is brought to a white heat by coke, the blast through the tuyeres urging the fire. Whilst the convertor is being heated, opportunity is afforded to the spectator to inspect the reverberatory furnace adjoining, where the pig-iron is melted. The convertor being sufficiently heated, it is turned upside down, and all the unburned coke falls out. The mouth of the vessel is then brought into a level with the spout of the furnace, which is tapped, and the molten pig-iron runs into the convertor. When the proper charge of pig-iron has been run in (in Messrs. Rowans' case, about 3½ tons), the blast is turned on, and the vessel quickly moved into a vertical position, with, of course, the mouth upwards. The blast, at the pressure of 15 to 18 to the square inch, now rushes into the fluid metal from each of the forty-nine holes of the tuyeres, producing a most violent agitation of the whole mass, and most intense combustion,—the combination of silicon and carbon with the oxygen of the air is eliminating most combustible gases. While this is going on, all the impurities in the pig-iron are carried off in brilliant flames, which illuminate the whole building, throwing brightest gas-light into the shade. During the period of blowing, several important changes in the flames take place, by which the state of the metal is known, and at the end of fifteen to eighteen minutes a decided alteration is observed, when the vessel is immediately turned down to the horizontal position, and the blast shut off. Thorough decarbonization is now effected, and the mass is now in the condition of malleable iron.

To make it into steel of the required temper, a known quantity of carbon must be given back, and this is effected by running into the convertor, from another furnace, a proportion of melted magnesian spiegle iron, which completes the converting process. The convertor is then tipped further forward, and the fluid steel is run in a rough casting-ladle, which is made of a similar material with the convertor, and is carried by a hydraulic lift to the moulds which are prepared for the steel. Instead of tilting the casting-ladle when pouring into the moulds, there is a conical valve of fire-clay in the bottom, which is easily and safely worked; and this plan of tapping the metal from below has the advantage of preventing scoria or other floating impurities from running into the moulds. By this process from 1 to 10 tons of iron may be converted into cast steel in less than half an hour; with the loss of from 18 to 20 per cent. on the weight of the pigs, at comparatively small cost for fuel. Indeed, Bessemer steel is now produced by Messrs. Rowan and others at best iron prices. It is not to be wondered at, therefore, that it is now coming into pretty general use

where malleable iron has hitherto been employed—such as for railways, axles, rails, and tyres, piston rods of steam engines, plates for steam boilers, and more recently the hulls of ships. For the latter purpose Bessemer's invention will doubtless be found most valuable, the toughness and strength of his steel enabling much thinner plates to be used in shipbuilding, for mercantile purposes, and in men-of-war, enabling them to carry invulnerable armour-plates.

When I visited the works, they were about converting and casting some large blocks, which were afterwards to be hammered and rolled into railway tyres, and I was much struck by the facility with which the whole ponderous machinery was worked by one man, who set the thing in motion by turning a small crank. The only difference in the process now, from what it was when the preceding report was written, consists in the fact that only hematite iron is used along with the speigle. The charge that was run out during my visit produced eight or nine blocks, which were taken and placed under a large Naysmith's steam hammer, and from thence to the rolling mills, where they were converted into solid railway steel tyres. Mr. Rowan informed me that the produce of the furnace charged at 5.30 A.M., and run out about 9 A.M., was deliverable in the form of tyres next morning. The proprietor also carries on to a very great extent the manufacture of composite railway wheels and buffers for the English market, and which have a very high character, and for the production of which a great portion of the work is specially arranged. Mr. Rowan complains a good deal that, when cast in large blocks, the Bessemer steel is rather uncertain in its quality; but time and practice—if the theory is quite a correct one—will overcome all difficulties.

Whilst upon the subject of Bessemer steel, I cannot refrain from mentioning a visit which I had the pleasure of making this season to the extensive works of Charles Camell & Co. (Limited), Sheffield, who are now about the largest makers in England. For one work alone, the Cyclops, they pay £400 per week for license fees to Mr. Bessemer. The Cyclops works are about the oldest in Sheffield, and when first erected stood alone, surrounded by grass fields. The locality is now one of the most populous, and encompassed by streets on all sides. The progress of the work has been most steady in its increase, and there is hardly now one in Europe to beat it. Much has been said about foreign works of the same kind, especially steel works, and which have been held up as beating anything in England; but this work, entirely supported by private enterprise, competes successfully with those huge enterprises supported by foreign governments and the influence of large capital. The principal products of this work have been

steel instruments of every description, also large orders by home and foreign governments for railway plant, engines, armour-plates, &c. The works cover upwards of 50 acres, and constantly employ between 3,000 and 4,000 men, with a working capital of £1,000,000. Twenty converting furnaces are constantly going night and day for the production of cement steel, 500 melting furnaces being employed for the purpose. A large quantity of this material is used for the manufacture of cannon shot and shell, railway tyres, forging, and all kinds of steel required by engineers. Lately the manufacture of iron has been added, sixty puddling furnaces being employed, with all the required machinery: 200 tons of armour-plates can be produced weekly.

The long planing shop, where the finishing of the armour-plate is carried on, is one of the finest in England, and contains twenty large planing and slotting machines. A large travelling crane is also employed, capable of lifting 20 tons. Plates fourteen inches thick have been rolled, and a steam portable crane moves about the yard for lifting extra weights.

These works also produce locomotive frame-plates, and everything, in fact, that is required by a locomotive or steam engine. The average turn out of springs alone for locomotive and other carriages of every description is at the rate of 120 per week. These springs are severely tested by hydraulic power before being sent out.

This same company have also enormous works at Grimsthorpe and Penriston.

HYDEPARK LOCOMOTIVE WORKS, SPRINGBURN.

I now come to a very important work, and one which I approach with some diffidence, as requiring some skill to describe the elaborate and beautiful organization by which the Locomotive, from the commencement, is gradually arranged in all its minute parts, and ultimately turned out in its finished condition. Although I feel aware that many know so much more than perhaps I can describe or explain, yet I think it necessary, before going into details, to devote a few minutes to the history and origin of the locomotive, and refresh our memories with the brief outline of the leading facts connecting the earliest attempts with the present exquisitely sensitive and delicate, but at the same time powerful structure, which has been the result of much industry and labour.

In 1763 a man named Cugnot, a Frenchman, was the first who exhibited a model of a steam carriage; but it altogether wanted regulating power, and was ultimately laid aside. Next, in 1784 and 1786, we have Symington, the inventor of the steam-boat, who made

a working model for road purposes, but it never was practically used; in 1784 we have also William Murdoch, who made a working model which ran away from him at the first trial, and nearly frightened the clergy-man of the parish out of his senses, when he observed it coming along the road like a fiery monster. Others followed with like success; but the first practical engine was constructed by one Richard Trevithick of Cornwall: this was sent to London for exhibition; but, from the then bad state of the roads, had no great practical result. This took place in 1802. In 1804 this gentleman again tried his hand, and applied his engine to the iron way, as it was then called, horse traction having only been used up to that date. The trial took place at Merthyr Tydvil, in South Wales. In 1815 he further improved his invention, but ultimately seems to have abandoned the whole matter, an idea having become fixed in his mind that under no circumstances could heavy weights be dragged behind the engine, in consequence of the insufficiency of the grip or bite, and that the only result would be slipping round of the wheels upon the rail. In 1811 a Mr. Blenkinsop took the matter up, and tried a tooth trail. This system, with modifications, was worked, and the engine certainly used, and with some profit, for dragging coal waggons, although at a very slow speed. But I must now come to the first truly practical experiments, which were inaugurated about this time on the Liverpool and Manchester lines. For many years the locomotive departments of this and other early lines had to go through many difficulties to meet the requirements of a fast-increasing traffic. And unfortunately it was not with a calm deliberation that the various improvements and experiments could be carried out; for even when imperfections were known to exist, the alterations had to be executed hurriedly at night, in order that the requisite power might always be present to meet the daily requirements of business. These struggles have gradually culminated in the present almost perfect illustration of applied industry.

The locomotive engine, which in the early days had only four wheels, and cost on the average £550, is now superseded, both on the broad and narrow gauge, by the six or eight-wheel engine, costing £2,500, and weighing 20 or 30, or even 40 tons. The first successful engine on the Liverpool and Manchester line was only required to draw three times its own weight. One of the powerful engines on the Great Western line will now drag, without difficulty, thirty passenger carriages, each weighing 5½ tons, at 30 miles an hour. The express trains go at from 65 to 75 miles per hour, and the goods engines, as now constructed, can drag 500 tons at 20 miles per hour. And, in fact, to such an extent has this gone, that a passenger train

of 120 tons can go at the speed of 60 miles per hour upon easy gradients. The evaporation of the boiler of such an engine is equal to 1,000 horse-power, or 33,000 pounds per horse; and the effect of power, as measured by the dynamometer, is equal to 733 horse-power. The weight of the engine is 45 tons, and in working order the tender is 18 tons. The diameter of the cylinder is 18 inches, the length of the stroke 24 inches; the diameter of the driving wheel 8 feet, and the maximum pressure of the steam 120 pounds. The average consumption of coke with an ordinary load, including stoppages, is 20 lbs. and upwards per mile. Some of the narrow gauge engines are even more powerful—for instance, one built by Crampton's Patent had 2,285 feet of heating surface, being 270 more than the largest engine on the broad gauge. The engine itself weighs 32 tons, and the evaporation whilst at full work is equal to 1,140 horse-power, and the pressure of steam 120 on the square inch.

To illustrate the power and speed of these engines is not easy, but one or two facts may be mentioned. When a speed of 70 miles per hour is to be attained, a space must be passed over of 105 feet per second; that is to say, 35 yards must be traversed between each tick of the clock. Two trains passing each other, the relative velocity will be doubled, so that, if one of them be 70 yards long, the second only will be taken up in the act. Dr. Hutton, who made some experiments in regard to the velocity of cannon balls, ascertained that the speed of one having a range of 6,700 feet is a quarter of a minute, which is at the rate of 300 miles per hour. A railway train, therefore, moving at 75 miles per hour, has one-fourth the velocity of a cannon ball.

Having thus far trespassed upon your time, I shall now attempt to describe the works of Springburn, and give a faint idea of how, from the 5,416 multitudinous parts, order is made to arise. The management of the Hyde Park Works is very perfect, insomuch that, commencing at one side, you can trace the raw material through every phase consecutively, up to the finished engine, ready to start and fulfil its duty, by a branch line to the North British, leading out of the works, and thence to any part of the world. On first entering, I was taken to the smithy, which contains something like sixty fires, including wheel fires and spoke furnaces. You also see nine steam hammers from 3 cwts. to 54 cwts., all hard at work, as also seven forge furnaces. Here you have brought into shape all the component parts of the locomotive formed of iron—such as piston rods, axles, connecting rods, and others too numerous to be detailed. In fact, this

shop produces from the largest to the smallest forging required in the manufacture of the locomotive. The steam hammers, directed by boys, do the work of giants, and so perfectly are they under command, that the largest mass, weighing many tons, can be shaped almost ready for turning, or so controlled that the force would hardly crack an egg. But the work that they generally have to perform is forging and welding the crank-axles.

The wheel-making is a process of much ingenuity and interest, all the more so from the absolute necessity that the work should be of the most perfect description. Each wheel is composed of above 100 small pieces forged and welded together, the material having previously undergone many heatings and hammerings subsequent to its formation from scrap iron. The tyres used in this establishment are not generally made by the proprietors, as in most cases they are made of Bessemer steel, and furnished by such large works as Messrs. Camell & Co., Brown & Co., of Sheffield, as also Messrs. Rowan & Co., of Glasgow. They are furnished in the rough state direct from the rollers; but at the Hyde Park Works they are turned, polished, and accurately fitted, each to its particular wheel.

The Boiler Shop.—In this pandemonium, called the boiler shop, it was difficult to ask a question, the noise was so deafening; but here it is that the boilers—those ingenious and intricate expositors of steam—are framed and put together. The sheet iron destined for the work is here cut and shaped into the proper size, drilled round the edges for the rivets, and put together by a most ingenious steam contrivance which clinches on both sides, making a most beautiful job, and leaving no hammer marks. The rivets are heated by boys, and being rapidly inserted into the plates, which swing vertically, the man in charge can rivet three in the short space of three or four seconds. This is a great improvement on the old process, both as to quality of finish and regulated strength. In the production of a boiler lies one of the great difficulties, for the strength and accuracy of the fittings must be perfect. The riveting of all the internal tubes should be able to stand the extreme tests to which the boilers are put before being turned out. It is the practice in this establishment to do this three or four times during its manufacture: first, when it comes from the hands of the riveters, afterwards, when placed upon the sole-plate, the full hydraulic test is applied. If all seems perfect, steam is then tried before the outer coverings are put on. And Mr. Neilson informed me that, as a result of this caution, he had never heard that any explosion had taken place with regard to any of their engines.

The Foundry.—This is a compact and well-regulated place, having

a small engine devoted to its use for the purpose of working the cranes, &c. Here is carried on the casting of all the small pieces, as also the cylinders, the iron used being a special mixture, which is mixed and re-melted till brought to the proper strength and hardness. I am thus particular in giving details which may appear perhaps too simple; but if you consider the fearful risks involved in the manufacture, and responsibility of engines working at such a high power, too great credit cannot be given to those who are thus careful in the selection of their material. Adjoining this shop is the *Brass Foundry*, the metal being carefully compounded and reduced in crucible furnaces for the casting of bearings, &c.; and through the next wall is the *Coppersmith Shop*, where pipes and domes are made and finished.

The Fitting Shop.—This is the largest in the establishment, having accommodation for 400 or 500 men, all busily employed in boring, planing, shaping, slotting, drilling, &c. Here you see the turning of the crank-axles, and the surface turned out from the different planing machines, so perfect, from the superiority of the machinery employed, that no trouble is almost afterwards required in putting the parts together. And as a consequence of this, you have a perfection of fitting which no manual labour ever could produce; and this being so essential in the manufacture of the locomotive, it is matter of much consequence to realise this apparent perfection.

The Erecting Shed.—This is one of the most important shops in the establishment, for here the 5,416 parts are adjusted, fitted, and ultimately brought into discipline, and it may easily be conceived that to bring such combination into harmony, the utmost accuracy and completeness of finish must be observed. The failure of one screw or rivet, or the bending of the smallest rod, might not only cause the ruin of the costly fabric, but occasion destruction of life to a terrible extent. Stephenson well observed that a locomotive required to be put together as carefully as a watch. The process being completed, the engine is taken to the *Painting Shop*, where it is decorated according to the taste of the company for whom it is destined.

The cost of engines built in this establishment, as in every other, varies, according to size, from £1,000 to £2,000, and the works themselves are adapted for the production annually of 100 engines. Having already, in my preliminary observations, described the speed and power of engines, it will not be necessary to recapitulate: sufficient for my purpose to state that the Hyde Park works now compete with the largest establishments in the world, both as to finish, power, speed, and economy of fuel. When the works are fully employed, the usual corps of men is somewhere about 1,400, and if regularity, perfect organiza-

[redacted]

tion, can secure success, it must fall to the lot of the enterprising proprietor, who was the first in Scotland to involve a large capital in this special department.

I have much pleasure in taking this opportunity of acknowledging the extreme courtesy with which I was met by the proprietors of the different works—viz., Messrs. Randolph & Elder, Messrs. Rigby & Beardmore, Mr. J. M. Rowan, and Mr. Walter Neilson.

When so much of importance has to be described, I may perhaps be accused of having gone too elaborately into the description of the Works I brought before you; but I trust at some future period, with your permission, to continue this interesting topic, and take up some of the other branches of that commercial enterprise, in all the different materials, which has given Glasgow a world-wide reputation.

Before parting this evening, I think I would not be doing my duty were I not to make mention of a circumstance, which must be a source of pride and gratification to each individual member of this Society. Since we last met, the greatest scientific work of the age has been completed; and we now can fairly say with Puck, "I'll put a girdle round the world in forty minutes." And when one takes a retrospect of the determined and persevering energy with which one of our most valued members devoted his time and energies to the elucidation of the difficult theories involved in this question, and, by his ingenuity and true philosophy, carried out to a successful issue those beautiful instruments which, without doubt, have been the main cause of the perfecting and bringing to a successful conclusion the great wonder of the age,—I need hardly say I allude to our late respected President, Professor (now Sir William) Thomson,—we must all feel honoured. It must be fresh in the memory of all, the firm determination with which he, session after session, placed before us each step of his great discoveries, and the patience with which he used to explain to us his beautifully simple but still complicated instruments. I might well be excused were I to go into a slight history of his connection with the Atlantic Telegraph, from its first beginning up to its present successful result. But, as I truly hope, I may be able to persuade Sir William, on an early evening, to give us his own experiences, I shall not trench upon what is so truly his own ground; but I am certain you will all agree with me in saying that he well deserves the honours that have been showered upon him; for a more true and high-minded gentleman it would be difficult to find. I trust he may be long spared to be an honour to his native city.

II.—*On the Water of the River and Frith of Clyde.* By Dr. R. Angus Smith, F.R.S.

Read December 5, 1866.

I lately adopted a mode of examining water, by which is seen, as I believe, the relative amount of organic matter readily decomposed and the amount actually decomposed, or the amount of putrid gases resulting from decomposition, and other matters. When this was done, I thought it would be interesting to test the Clyde by the new process, and for that purpose obtained specimens at various points from the Broomielaw to the sea, afterwards continuing till the vessel came near Liverpool.

The column to observe is No. 1 of either series. This gives the amount of oxygen consumed instantly, and corresponds, as I believe, to the amount of decomposed matter which has left in solution putrid gases, whatever these gases may be. I shall suppose them to be sulphuretted hydrogen in minute quantities, or compounds of sulphur or organic substances only. In the part of the Clyde near Glasgow all these compounds will probably be found; as we go near the frith the sulphur will be fully oxidized, and the organic compounds only will be the active agents in taking up the oxygen, as they oxidize more slowly.

The amount of oxygen is calculated from the amount of permanganate employed. The method is an extension of the original idea given out by the late Forchammer many years ago; the amount of water used was 100 cubic centimetres, or 1,544 grains. The permanganate required to give perfectly pure water perceptible colour was in every case subtracted from the number obtained.

The examination of the Clyde is not perfect, but there is much to be seen that is interesting. Specimens ought to have been taken higher than any town on the river. They begin below Bothwell, or one and a half miles above Cambuslang, about six and a half miles above the quay at Glasgow. We there find the oxygen required for the water to be 0·000045 per cent. by weight, or equal to 3·15 grains per 100 gallons. When we come to the Broomielaw at Glasgow, we find the amount of oxygen demanded has risen to 0·00022 per cent. by weight, or 15·40 grains per 100 gallons.

We then see it steadily diminishing, and when we arrive at Port-Glasgow, twenty miles down, it has become equal to that at six and a half miles above Glasgow. If we had taken the north side, below

Dumbarton, instead of the south, on which the vessels sail, the difference would have been greater.

Leaving Greenock, and turning the point, we have a rapid change, and when off the west of Gourock Bay, at the distance kept by the Liverpool vessels, the amount of oxygen demanded fell to 1·05. Off Wemyss Bay the fall is remarkably low, but it is not always so, as it depends on tide and wind. The current seems to run down the centre, whilst the ocean water comes up the coast. The wind and floods may modify this.

We have to observe a complete change about Gourock or the Cloch, if we keep to the south and east side, but if we keep to the north the utmost change is perceptible not far off Helensburgh.

This wide space of water reduces the amount of oxygen required to 0·00001 per cent. by weight, or 0·7 grains per 100 gallons.

We are now led to ask if the purest state of the water is attained here. We may go farther towards the open sea, and find that the amount required falls to 0·000005 per cent. by weight, or 0·35 grains per 100 gallons. This is at Arran, where the sea is more extensive, and oxidation even of the peaty matter from the hills is effected. We observe this to continue till the entrance of the North Atlantic is passed; and as we come south to a narrower channel, the amount of oxidizable matter increases, and keeps up steadily till we arrive at Liverpool. No specimen was taken nearer than seventeen miles from Liverpool.

The point 0·00001 per cent. oxygen by weight, or 0·7 grains per 100 gallons, is an important one to examine. We find that at Dunoon, Innellan, and Holy Loch, this amount may be called permanent. We may ask why does it not fall lower, and is it true that the sewage of Glasgow affects the frith until we arrive at Lamlash, and obtain the number 0·000005 per cent. oxygen by weight, or 0·35 grains per 100 gallons.

We observe that the amount of purification is remarkably rapid whenever the Clyde widens, as at Greenock to Gourock; so that even near Helensburgh the number has fallen to 0·0000125 per cent. oxygen by weight, or 0·875 grains per 100 gallons. This change from 0·000045, or 3·15 grains per gallon, has been effected in the space of about two or three miles. Is it possible, then, that the slight further change required demands all the wide distance between Greenock and Arran? I think not. The reason we do not find the number for oxygen falling below 0·00001 per cent. by weight, or 0·7 grains per 100 gallons at Holy Loch, Strone, Dunoon, and Cove, may be seen by examining the lochs. We there find that they themselves send in water with the

oxygen number 0·00001 per cent. by weight, or 0·7 grains per 100 gallons. At this point we must remember that the oxygen may not all be required for putrid matter, and that the streams contain some peaty matter which may also instantly require oxygen. At any rate we know this, that for whatever they do require it, it can be for the destruction of no very unwholesome matter in the water, as the Highland streams must be considered free from such taints. There is a little of the soil of cattle when the weather is wet, but perhaps none when the weather is so dry that the water must pass through the earth before reaching the bed of the stream.

We cannot then expect the sea water to demand less oxygen than the streams and lochs that affect it here. In other words, we discover the influence of the land on the drainage from the hills, even when all trace of the influence of Glasgow has gone. This is, I believe, the reason that, according to circumstances such as currents and weather, we observe the water of the frith sometimes as pure as the deep sea, and at other times less so, at Wemyss Bay for example. The table does not show this very well. This will happen at other parts of the coast when the streams enter purer than the usual sea water there, as for example at Dunoon and Innellan, where the water flows from streams equal, when not in flood, to the deep sea water. We see the influence of the coast also as we go on towards Liverpool, and between the Isle of Man and Lancashire.

I consider that 0·00001 per cent. oxygen by weight, or 0·7 grains per 100 gallons, marks the limit of the Glasgow sewage. If the sewage were not all expended the amount would be greater, because the lochs and their streams do not show a higher number. Whenever the water of the frith falls down to that of the streams, we are, as I suppose, receiving the air at least as pure as the air above the streams on the hills, and may be contented.

Some people will perhaps demand more than this number. I have not made examinations far enough towards the ocean to enable me to speak of it; but it seems to me that the numbers 0·00001 per cent. by weight, or 0·7 grains per 100 gallons, of the streams, and 0·000005, or 0·35 grains per 100 gallons, mark two very important points. The first characterizes air which is certainly most wholesome and agreeable; the second is that ocean air which some persons require, but which to others is said to be too strong. Of the meaning of this expression "too strong" I will not attempt to speak, but it is very much used by a great public, and it must have a wide foundation. It is apparently certain that to some it is important not to have the extremely strong or perhaps purest air.

The observations here made seem most curiously to coincide with general observation. The current of the Clyde keeps to the south, and we see that the water changes less rapidly there. At the north the direct current is not much observed, and the influence of the lochs or fiords is seen. The water from them presses against the Clyde current, and aids in keeping it to the south-east. This has influenced the building of the numerous residences on the Argyle side, lining all the coast. The population has not found it needful to make a chemical analysis, but has seen where the purest water was with the unaided eye. A mode of observation apparently quite as good as chemical analysis is the examination of the rocks on the coast, the dark and dirty aspects of these in impure water being very striking.

I am not, however, inclined to look on my work here as useless, although common observation has done its part so well. I believe it to be of great importance to us to be able to apply tests so as to prove the truth of popular belief, which is formed by long and tedious processes. With these little chemical experiments we may do the work in an hour which the instincts and expensive experience of whole generations were required to perform.

I believe that in these results we have a method of examining the coasts which will be valuable to us as a sanitary police. It can be applied wherever there are sheets of water.

I will not say that these numbers represent the relative wholesomeness of places on the land and on the sea; but I think it probable that they may fairly be used as comparisons of places on the sea only. When the land is in question there are many sources of emanations to be considered.

To be complete, the examination ought to be made for several years. Last year was not an average one. There was a great deal of rain.

There are floods in the Clyde which bear down so much water that the brown peaty matter is readily seen below Dunoon; but on these occasions there is no fear of putrid matter, as the water carried down is so abundant. At the same time there will be a certain degree of purity less at the sea entrance, corresponding to the greater degree of purity caused by the flood at Glasgow. By this it is not meant that the water as low as Cloch or Dunoon is affected by more than an infinitesimally small amount of the impure matter which is washed past Glasgow in floods, whilst at other times the effect has ceased, as beforesaid, at least on the north. There is, however, a little muddiness observable for many miles, and this will be found even in places where the oxidation of all the readily transported matter has taken place. Even where total oxidation has occurred, a little muddy matter remains. It consists

chiefly of earthy bodies which may float until they obtain a suitable place for deposit.

It would be interesting to find the time needful to perform this change on the organic matter of Glasgow. We see that the great bulk of the purification is performed in the fresh water. At Dumbarton it must be rare, indeed, that anything can be known to exist in the water from its smell. When the Clyde passes into the estuary, which may be said to begin at the rock of Dumbarton, the change is effected by spreading the water over many square miles, and preparing it effectually for those who enter the sea and loch regions below Helensburgh. Below this point it is not a river on which we are, but an arm of the sea, protected on all sides from oceanic waves. The water bears the qualities of coast water in proportion to its excessive length of coast. This quality must exist more or less on all coasts which have fresh springs running into them. In cases where the rivers are large or numerous and the tides small, the evil of the meeting of fresh and salt water is well known. On the Frith of Clyde no large bodies of fresh water are found after we leave the mouth of the river portion of the Clyde; and, as observed, these streams are remarkably pure, or, if coloured, it is only by peat water. The absence of great rivers is therefore an advantage of an important kind to this coast.

I was extremely glad to be able to speak thus favourably of the districts which are so agreeable to me; but I am certainly desirous of speaking still more favourably of some which are too much affected by Glasgow. They may be constantly deteriorating. This is a very serious matter, not merely for owners of property on the frith, but for all the inhabitants of Glasgow, who are favoured more than those of any other large city in the kingdom by a ready access to this wonderful sanatorium.

In making these remarks, I take for granted that the air above the water will be pure in inverse proportion to the amount of volatile oxidizable matter.

We must not, however, forget that if the river is rendered impure, the town is proportionately purified. And we must also remember that the work done in the water is enormous. If the refuse matter were allowed to evaporate into the air, and so collect its oxygen, the act of purification would go on in the air we breathed; but now the water draws in oxygen, and as soon as it is consumed it draws in more. Even the air of the river is, at the distance of a few miles down, purer than the air of many towns; and when the spaces widen, the case is stronger still.

I have sometimes inquired if we obtain in fish an equivalent of the manure sent out, and if it is not easier to gather the crop in boats, than to put the manure laboriously on land. I find, however, that the loss by putrefaction, oxidation, and evaporation, is so rapid that there is no hope of it being used profitably. In one current examined—a very deep one, and much stronger in sewer matter than the Clyde ever is—the whole nitrogen had disappeared in three days—the greater part in much less.

Whilst the chief object of my paper is to show a new mode of examining climate as well as streams, I am glad to be able to say that Glasgow has failed to destroy the purity of the frith beyond the points indicated, and that we may still rejoice in our summer dwellings. It is, however, not to be forgotten that the effects are found too far away, and that twenty-two miles of mischief is more than abundance. It is to be hoped that the cautious policy of the city, in waiting for the best method to be adopted for using its refuse, will enable it to carry out that future work, whatever it may be, with the greatest success.

I have referred only to the first column. It indicates the amount of permanganate decomposed instantly. This represents the gases of decomposition such as most readily pass into the atmosphere.

It will not be supposed that this is the only offensive matter.

The second column represents the amount when acid is used to assist the oxidation. I suppose this to represent the amount easily decomposed.

The third column represents the amount of oxygen used by the organic matter in twenty-four hours. I do not at present see its use. I do not think this at all a fair mode of representing the impurity of water, unless we are sure that the quality does not change. We see this by the numbers above and at Glasgow bridge. The purer water has, by this mode of representing it, the appearance of being equal to the worst. The only column that gives the continuous increase and decrease exactly as the senses find the case to be, is the column No. 1.

It is not, however, meant that the gases of decomposition are the only evils to be apprehended from impure water; but so far as I know, they are those that affect the air principally. The most dangerous bodies where the water is to be drunk—not the case on the Clyde—may exist in the water itself, and it is possible that they may not be affected by the process of column 1.

I am not inclined to draw any important conclusion from the total organic matter without inquiring its character. At present I believe we are quite safe in attending to the conclusions here given in column 1, with such precautions as I have elsewhere indicated: when, for

example, nitrates, nitrites, sulphites, &c., are present. By-and-by we shall learn the true mode of dealing with the third column.

	Time.	Oxygen per 100 Gallons required to oxidise			Oxygen per Cent. by Weight required to oxidise		
		I. The De-composed Organic Matter.	II. The easily Decom-posable Organic Matter.	III. The Total Organic Matter.	I. The De-composed Organic Matter.	II. The easily Decom-posable Organic Matter.	III. The Total Organic Matter.
6¼ miles above Glasgow, ...	Aug. 15, 1866. 3.30 P.M.	3·15	11·55	176·40	0·000045	0·000165	0·002520
5¼ miles above Glasgow, ...	3.15 P.M.	4·55	9·45	175·00	0·000065	0·000135	0·002500
5 miles above Glasgow—Cambuslang,	3 P.M.	5·95	11·20	174·65	0·000085	0·000160	0·002495
Broomielaw, ...	1.30 P.M.	15·40	22·05	172·90	0·000220	0·000315	0·002470
Mouth of Kelvin, ...	1.20 P.M.	11·55	18·20	172·55	0·000165	0·000260	0·002465
100 yards below Renfrew, ...	1 P.M.	10·85	25·55	148·75	0·000155	0·000365	0·002125
100 yards above Bowling, ...	12.45 P.M.	4·55	8·05	168·70	0·000065	0·000115	0·002410
At Dumbarton, ...	12.30 P.M.	4·55	6·65	164·85	0·000065	0·000095	0·002355
Between Greenock & Port-Glasgow,	12.10 P.M.	3·15	4·20	138·25	0·000045	0·000060	0·001975
Gourock, ...	July 25, 1866. 12.55 P.M.	1·05	1·75	...	0·000015	0·000025	
Cloch, ...	12¾ P.M.	1·05	1·57	32·55	0·000015	0·0000225	0·000465
Inverkip, ...	12½ P.M.	0·70	1·40	23·80	0·000010	0·000020	0·000340
Wemyss Bay, ...	12¼ P.M.	0·35	0·87	25·55	0·000005	0·0000125	0·000365
Opposite Toward, ...	12 NOON	0·70	1·05	18·11	0·000010	0·000015	0·00025875
Between Toward and Cumbrae, . .	11½ A.M.	0·70	1·05	...	0·000010	0·000015	
Opposite Cumbrae Lighthouse, . .	11 A.M.	0·70	0·87	...	0·000010	0·0000125	
Opposite Lamlash, ...	10¼ A.M.	0·35	0·70	21·17	0·000005	0·000010	0·0003025
188 miles from Liverpool, ...	10 A.M.	0·35	0·87	22·05	0·000005	0·0000125	0·0003150
176—Ailsa Craig, ...	9 A.M.	0·35	0·70	...	0·000005	0·000010	
160—From Liverpool, ...	8 A.M.	0·35	0·87	21·17	0·000005	0·0000125	0·0003025
131— "	6 A.M.	0·52	0·87	...	0·0000075	0·0000125	
103— "	4 A.M.	0·52	0·87	29·05	0·0000075	0·0000125	0·0004150
74—North of Isle of Man, ...	2 A.M.	0·70	1·05	20·30	0·000010	0·000015	0·0002900
46—From Liverpool, ...	July 24, 1866. 12 NIGHT.	0·87	1·22	...	0·0000125	0·0000175	
32— "	11 P.M.	0·87	1·40	...	0·0000125	0·000020	
17— "	10 P.M.	1·05	1·75	...	0·000015	0·000025	
Off Greenock, ...	Aug. 13, 1866. 5.10 P.M.	1·40	1·57	...	0·000020	0·0000225	
Off Helensburgh, ...	Aug. 15, 1866. 6.35 P.M.	0·875	1·22	...	0·0000125	0·0000175	
Off Gourock, ...	Aug. 13, 1866. 5 P.M.	1·22	1·57	...	0·0000175	0·0000225	
Between Kirn and Gourock, ...	Aug. 13, 1866. 6 P.M	1·22	1·40	...	0·0000175	0·000020	
Arrochar (Loch Long), ...	July 30, 1866. 1.30 P.M.	0·70	1·05	...	0·000010	0·000015	
Off mouth of Loch Long, ...	Aug. 15, 1866. 6.55 P.M.	0·875	1·40	...	0·0000125	0·000020	
Head of Loch Goil, ...	Aug. 17, 1866. 1.5 P.M.	0·875	1·05	...	0·0000125	0·000015	
Entrance of Loch Goil, ...	4.30 P.M.	1·05	1·22	...	0·000015	0·0000175	
Head of Holy Loch, ...	Aug. 1, 1866. 2.45 P.M.	0·70	1·57	...	0·000010	0·0000225	
Middle of Holy Loch, ...	2.30 P.M.	0·52	1·05	11.44	0·0000075	0·000015	0·0001635
Dunoon (several 100 yds. from shore),	July 31, 1866. 1 P.M.	0·70	1·22	...	0·000010	0·0000175	
Dunoon (about 800 yards from shore),	1.15 P.M.	0·70	1·22	...	0·000010	0·0000175	
Off Dunoon, ...	Aug. 16, 1866. 1 P.M.	0·70	0·875	...	0·000010	0·0000125	
Stream at West Bay, Dunoon, . .	Aug. 20, 1866. 1 P.M.	0·52	0·70	...	0·0000075	0·000010	
Stream between Innellan & Dunoon,	12 NOON.	0·35	0·52	...	0·000005	0·0000075	
Between Innellan and Dunoon, . .	Aug. 16, 1866. 1.8 P.M.	0·875	1·05	...	0·0000125	0·000015	
Off Innellan, ...	1.15 P.M.	0·70	0·875	...	0·000010	0·000012	
Off Wemyss Bay (high tide), . . .	Aug. 9, 1866. 12 NOON.	1·05	1·22	...	0·000015	0·0000175	
Off Wemyss Bay (low tide), . . .	4.30 P.M	0·875	1·22	...	0·0000125	0·0000175	

The PRESIDENT said he was sure they were all delighted with the paper which had just been read to them, and he would be glad to hear remarks by any of the members. He was certain there were many gentlemen present who could give corroborative evidence and additions to the information which had already been received.

PROFESSOR THOMAS ANDERSON said that Dr. Smith had given them additional evidence of the condition of the river, the effect upon it by the city of Glasgow, and its gradual improvement as it descended towards the sea. The information which had been imparted to them, he was sure, they had all received with the greatest possible interest. He confessed that he had used the permanganate to a considerable extent, but he had not the highest degree of confidence in the results derived from it. He thought the conclusions to which it led must be looked upon as those of a very general character. At least, according to his observation, when the quantity of impure water was large, the results given by that re-agent were far from satisfactory, and he had lately banished it altogether. It was a curious fact that the water of Loch Katrine, the purity of which they so much prided themselves upon, discoloured the permanganate to a greater extent than the water which was supplied to London.

MR. ALEX. HARVEY wished to know what effect would be produced on the river by the removal of the weir. At present, very little of the sewage of the city passed above the weir, and he would like if Dr. Smith could tell him what would be the state of matters if it was removed.

MR. WEST WATSON said, if he remembered rightly, Dr. Smith mentioned that the comparative number of grains at the Broomielaw was fifteen, and at the mouth of the Kelvin eleven or thirteen. He would like to know if these two observations were taken at the same condition of the river, as to ebb and flow; and he would also like to know whether the water tested at Port-Glasgow was drawn from the surface or from a depth; and, also, whether any distinction was made between spring tides, neap tides, and medium tides. He thought all these were subjects of very great importance.

MR. DAVID MORE said he might be allowed to tell some of his chemical friends, that the Clyde water with which they fed their boilers previous to the introduction of Loch Katrine water always left a thick deposit; and if this was allowed to remain for a month or two beyond the usual time of its removal, it was very dangerous, causing the boiler to burst. The boiler which he referred to was twenty-one feet long and six feet in diameter, and now with the Loch Katrine water it was only cleaned out every six months, and very little sediment was found in it.

Professor Gairdner concurred in the remarks made as to the great value of Dr. Smith's paper. There was no doubt, however, that the impurities demonstrated by chemistry were not at all of equal importance; to determine which of them are, or are not, injurious to health or comfort, other researches are required. The whole subject was full of difficulties; but it seemed highly probable that the day was coming when no town would be allowed to throw its impurities into the river. They must, of course, recognize the fact that the rivers of the country were to a certain degree the means of getting rid of impurities, and until the mode was shown of disposing of them better, they could not forbid people to throw them into the river. He believed, however, that science would yet show how this could be done, and then the legislature would forbid all fouling of streams. A great deal had been written in the Glasgow papers lately tending to put into the public mind the idea, that the purification of the Clyde was thrown into the background by other sanitary projects which were not so important. Now, the truth was that the distinct appreciable amount of injury which the Clyde committed on the population of Glasgow was almost *nil*. It was impossible to prove that the impurity of the Clyde was a cause of disease or death. Of course, Dr. G. added, he was not speaking in a sense opposed to the purification of the river, nor was he maintaining such a paradox as that a foul river was a good thing, or even not injurious; but so far as the great causes of epidemic diseases were concerned, they were to be sought elsewhere. The impurity of the river was not, he believed, to any demonstrable extent, responsible for epidemic mortality.

Dr. Smith, in reply to the various questions which had been put to him, said, if it was denied that the products of decomposition were unwholesome, they would meet with many difficulties. He did not suppose, however, that Professor Gairdner went so far as that; and if they said with him that these gases were not the causes of epidemic diseases, he thought they would be quite correct, but that they affected the health in some way he supposed they must all agree. With reference to what Professor Anderson said, he quite agreed with him that the results generally produced by the permanganate were uncertain; but his method of using it, he thought, was not subject to the usual objections. He did not rely on ascertaining the total amount of organic matter by permanganates, but he had the fullest confidence in comparative results. In answer to Mr. West Watson, he said that the specimens were all taken from the surface. The water tested between this and Dumbarton was taken at ebb tide, and that drawn from the Frith of Clyde was taken at many stages of the tide. The

difference in the water taken at high and low tide was not very decided; but that tested at high flood was considerably different from that taken at either of the two stages of the tide. Mr. Harvey's question was rather too serious for him to reply to at a moment's warning.

A vote of thanks, on the motion of the President, was awarded Dr. Smith.

III.—*On the Scientific Premonitions of the Ancients:* I. Greek Geology. By JOHN YOUNG, M.D., F.R.S.E., Professor of Natural History in the University of Glasgow.

Read December 19, 1866.

IT has been said that there is nothing new under the sun; and geologists have laboured to prove that, at least in the inorganic world, the melancholy conclusion holds good, that the thing which has been shall be again. But while these researches are received with approbation, it by no means follows that he who seeks to prove a similar proposition, "that which has been thought, the same shall be thought again," deserves an equal meed of praise. The progress of any important discovery has been well traced as rousing, first, opposition of the doctrine; next, denial of its originality; a last stage might be added—assertion of its previous universal acceptance. It is not true—it is not new—every one knew it before; so it slides into the common creed, and in a short time its date of discovery is known only to the learned. It is needless to recapitulate the many examples, preserved as curiosities of literary or scientific history, on which the cynical encouragement to original thinkers is founded. At present I would confine my remarks to one of the methods by which the second stage in the suppression of merit is attempted or achieved.

When the originality of Darwin's theory of the origin of species is contested, on the ground of its similarity to the views put forth by Lamarck, the critic proceeds to verify or disprove the assertion with care, conscious of the possibility of coincidence between two observers so near to each other in point of time, therefore so nearly on an equality in respect of the knowledge on which their speculations might be based. When, having made the inquiry, it appears that the two theories differ most importantly—that while Lamarck assigns to external conditions for their result the production of new organs, by inciting to new actions and habits, whereby the animal accommodates itself to an altered mode of life—Darwin sees in such altered condi-

tions circumstances unfavourable to the ordinary individuals of a species, but favourable to some few individuals possessed of particular variations of form or habit, which enable them to get on better among the altered surroundings—circumstances whose continuance admits of the increase of the specially favoured variety, while the less favoured congeners perish;—while the one makes the individual personally active in the morphological change, the other sees the chance-produced variation perpetuated by its suitableness to the new order of things;—while Lamarck finds in the present aspect of fauna and flora the sum of progressive transmutation, the lower terms of the series being maintained by a process of creation not easily distinguishable from spontaneous generation, Darwin is on his guard against assumptions of progression, because the data are manifestly imperfect, and, by the terms of his hypothesis, has no need for the introduction of any such arbitrary new creations;—when it is found that the two theories differ so widely, it is competent to doubt either the honesty or the capacity of him who finds in the latter a reproduction of the former speculation. At best, it is plain that he has mistaken formal similarity for essential identity, misled, perhaps, by the one common term in both—the influence excited by varying external conditions.

But the short interval of time which separates two authors does not of necessity involve that equality of knowledge which increases the chance of coincidence or complete anticipation. The ignorance of our contemporaries, in certain kinds of knowledge, may place them, relatively to us, as far back in respect to those kinds of knowledge as the revival of letters. The accomplished scholar of to-day may have less acquaintance with natural science than Aristotle, while a lively fancy may help him to utterances which at first sight resemble the discoveries of a later day. Restif de la Bretonne, a writer whose works are little known in this country, believed in a single progenitor in the animal and vegetable kingdoms respectively, whose descendants have differed under the influence of soil and climate. This is all; yet this, dignified as "a system," has been put forward as another anticipation of Darwin, by M. Fée, who, nevertheless, himself most justly describes the opinion as "hatched in a head purely literary," by one "who only wanted a little more reason to become a man of great eminence." The putting forward of a claim to scientific priority, on behalf of such an assertion and such an author, involves the double offence of depreciating an original thinker by means of one whose vague words only acquire meaning from the discoveries of him whose rival he is made.*

* *Geol. Mag.*, ii., 308.

The instances, however, are rare in which, unless passionate prejudice has for the time obscured reason, the insufficient knowledge of a comparatively modern writer is made the instrument of criticism so unfair. But it is not unfrequent that writers of weight, under the influence, perhaps, of some still lingering shade of mediæval scholasticism, seek in classical literature the forewarnings of modern science—seek, and, with the diligent searcher, this means to find, in some Greek or Latin author the knowledge hardly won by modern toil, yet lying there, we are asked to believe, lavishly scattered among the dust of ancient philosophy. Let me not be understood to seek in my turn to undervalue the solid good which we have inherited from the ancients, nor to depreciate the wonderful sagacity which marks many of their speculations, the clear foresight which, under unfavourable conditions as to the instruments of research, has enabled them to leave enduring monuments in science, whose ornaments we may indeed fashion differently, but the framework remains intact. What is really good needs not my commendation. What I would protest against is the attribution to them of knowledge it was impossible for them to have attained—knowledge which, when judicially investigated, is too often found wanting, and leaves upon the early philosophy unmerited suspicion, which ought rather to attach to its injudicious partisans.

If science be that "co-ordination of facts which describes the order of co-existence and succession in phenomena,"* it is evident that, proportionally as we recede into those times when the number of observed facts is least, the materials of science diminish, the observed succession of phenomena is more imperfect, the co-ordination of the facts, *pro tanto*, less worthy of acceptance. This holds true for all facts, especially for those whose just appreciation depends upon the fullness of collateral knowledge—above all, for those whose observation is not possible save by the aids which collateral inquiries can alone supply.

Much knowledge of the anatomy of the lower animals was undoubtedly within the reach of the Greek of the fourth century B.C.; but what is worthy of the name of comparative zoology was not even possible till a far later date. For not merely were the materials wanting which only a wide range of animals from all regions could supply, but even had they been accessible, they could not have been properly investigated till optics and chemistry had reached such a point of development as to provide instruments of research beyond the reach of the unaided senses. Nay more, could we conceive these aids to have been accessible to the ancients, it is impossible that sound anatomy or physiology could have been arrived at by their aid, so long as men's

* G. H. Lewes.

chief reliance in speculation was placed, not on the qualities of substances themselves, but on the verbal designations of these qualities, or on notions derived secondarily from these names. Thus continuous motion was held proved, because continuity is better than successiveness (if the word be allowed).

There will be hereafter occasion to return to the limits of the knowledge of the ancients; but at present it is well to consider a criticism which appeared in a semi-literary journal, whose staff includes more than one sound scholar:—

"*November* 3, 1865.—The curious in literary pedigrees have been tracing the parentage of Lord Palmerston's popular phrase, 'the fortuitous concourse of atoms,' with praiseworthy diligence. Those who were not aware that the words have been long in common use with students of old and modern philosophies, began by crediting the late premier with their origination. Then somebody said he must have learnt them from Dugald Stewart. Then South had the credit given him. Now we learn that Locke uses the phrase; while at the same time other writers quote from Cicero, to show that Locke only repeated what the old Roman had said before him. In fact, however, the idea embodied in the phrase is far older than Cicero, who, like the Roman philosophers in general, went to the Greeks for the materials of his abstract thought. The theory about these "atoms," whose chance combinations were supposed to have produced the existing organic universe, is to be found discussed in Aristotle's *Physics*,* and, what is just now most remarkable, in immediate connection with a theory identical with that which is propounded by Mr. Darwin in his *Origin of Species*. These atoms were believed to have been perpetually moving about from all eternity, forming a countless variety of organic combinations, out of which combinations only those survived to perpetuate themselves 'in their kind' which came off victorious in the universal conflict for organic existence. As described by Aristotle, the philosophers in question asserted that rain is not designed to make the corn grow, but that as rain happens to be the means of propagating the existence of grain, therefore grain, 'by the force of natural selection,' continues to flourish on the earth. If the rain had not happened to be ready to nourish the corn as soon as the fortuitous concourse of atoms made the first corn, grain would immediately have become extinct; till, in the never-ending shuffling of the cards, the same atomic combination would once more turn up. The real theory of Mr. Darwin is, indeed, summed up by Aristotle in a single sentence:—Ὅπου μὲν οὖν ἅπαντα συνέβη, ὥσπερ κἂν εἰ ἕνεκα τοῦ ἐγίνετο, ταῦτα μὲν ἐσώθη

* 198 b. Bekk., Ed. Maj.

ἀπὸ τοῦ αὐτομάτου συστάντα ἐπιτηδείως· ὅσα δὲ μὴ οὕτως, ἀπώλετο καὶ ἀπόλλυται, καθάπερ Ἐμπεδοκλῆς λέγει τὰ βουγενῆ καὶ ἀνδρόπρωρα. 'When all things happened to be produced just as it would have been if they had been constructed with a definite design (*ἕνεκα τοῦ*),* those combinations were preserved which happened to fall in with the general working of the whole; and those which did not thus fall in perished, and still perish, like the minotaurs and sphinxes that Empedocles speaks about.' It is impossible to translate the sentence except periphrastically, but its meaning is abundantly clear; and it shows that the Darwin theory is at least two thousand years old. The only difference between the old and the new speculation is, that while the new school use the word 'law' as indicating the relationships of the ultimate atoms, the ancient speculators attributed everything to a power which they called 'necessity.'"

Aristotle here states the opinion of his predecessors as a text for the teleological argument to follow. The antagonism which Empedocles found in all things, and which Chance alone seemed to his mind capable of counteracting, is probably specially aimed at by Aristotle, though the mention of "Intelligence" also, in the previous sentence, seems to involve Anaxagoras in the censure, widely though his conclusion differed from those of his successor and contemporary. The example cited, that of the growth of corn under the influence of rain, is this: rain is the "necessary" (*ἐξ ἀνάγκης*) result of the condensation of vapour; its fall the necessary result of that condensation; the growth of grain coincides with the fall; the two events are in accidental relation—growth is not the design (*οὗ ἕνεκα*) of rain. Again, the destruction of corn by rain is a similar coincidence, equally without design. The fitness of the teeth for their functions is a similar "coincidence." Then follows the passage quoted above.

To the arguments thus stated on behalf of his predecessors, Aristotle replies after this fashion:—

"It is impossible that this should be so; for these and all the products of nature are always, or for the most part, so produced; but none of the accidental or spontaneous products are so. For it does not seem by accident, or by coincidence, that rain is frequent in winter, but (it would be) if it happened in summer; nor are great heats at the dog-days accidental or by coincidence, though they would be so in winter. If, therefore, these seem to occur either as coincidences or from design (with an intention), and if such things cannot be held

* The accents are given as in the *Pall Mall Gazette.* It would be interesting to know the writer's reasons for the unusual form, and the covert scepticism it conveys.

to be mere coincidences or accidents, they must be for a purpose. But all things are so in nature (*i. e.,* for a purpose)—as even those who hold the above views would admit. There is, then, a purpose in the production and existence of all natural things."

This reply is a mere quibble. The reasoning is founded on relations which are as purely accidental (in the logical sense, not as meaning fortuitous) as could be chosen. Had he referred to the positions of sun and earth at these seasons, he would have improved the argument; but even then the doctrine of chance would not have been much shaken. The tenets he opposes have in truth a strong cast of that which now-a-days, as materialism or some other much abused, little understood spectre, guides all scientific research. Mr. Mill has very clearly put the modern creed; and, remarking upon "the revival of the doctrine that efficient or ultimate causes are within the reach of human knowledge," holds it "as a remarkable instance of what has been aptly called 'the peculiar zest which the spirit of re-action against modern tendencies gives to ancient absurdities.'"

Before quitting this subject, however, it must be mentioned that this refutation by Aristotle of the Empedoclean tenets seems to have been as inconclusive to himself as it is to modern readers. In his *Treatise De Partibus*, c. 2, he speaks of the gall bladder, and denies its influence upon the feelings, on the curious ground of its absence in some animals, and even in man. He therefore regards it as superfluous or "excrementitious," and not for a special purpose; and adds, "Nature, indeed, sometimes uses excrements for useful purposes; but we must not on that account seek a special purpose for all things, but, certain things being such as they are, many other things of necessity happen in consequence." It may be added, that if this passage did not rightly present the true practice of Aristotle, whatever were the formulæ to which he may have been led, he could not have reached that result to which, as an anticipation of the law of economy or correlations of growth, reference will be afterwards made.

The interpretation of these words, as a summary of Mr. Darwin's theory, results from one or two misapprehensions. In the *first* place, the growth of the corn under the influence of rain is said by the writer to be "by the force of natural selection." Darwin's terms are here totally misapplied, and, even if correctly used, are not warranted by the text. *Secondly*, the drift of the argument is inexactly stated in the paraphrase, "only those combinations survived to perpetuate themselves 'in this kind,' which came off victorious in the universal struggle for organic existence." No such struggle is hinted at in this or any other passage; it was not even dreamt of. The struggle, dimly

alluded to by Aristotle a few lines previously, was one between the forces of nature, which perplexed Empedocles. The conflict was for *individual* existence, not for that *serial* succession whose possibility, under adverse conditions, it was Darwin's object to illustrate. The speculations were, in fact, like all others of the period, rather directed towards explaining the origin than the persistence of all things. The hold which the doctrine of the Atomists maintained seems due to this limitation of its use, since, if followed out to its logical results, it must have broken down, as it did when extended in its application by Anaxagoras imperfectly, and more clearly by Aristotle. *Thirdly*, "the general working of the whole"—the phrase used in the above translation—vitiates the whole argument. The chaos of moral and physical forces, amid which chance alone seemed capable of producing a fitful order, was at variance with the idea of plan which these words manifestly suggest. That idea it is which Aristotle seeks to substitute for chance. The real meaning is, that those were saved which happened to be fit for the conditions to which they were subjected. The limitation of these speculations is evident from a subsequent chapter, in which Aristotle investigates "necessity," and states it to be a pre-assumed (ἐξ ὑποθεςέως) property resident in matter (ἡ ὕλη). This property, therefore, expresses the "relation of the ultimate atoms;" the relations of their aggregates is, with Empedocles chance, with Aristotle τὸ οὗ ἕνεκα ἐν τῷ λόγῳ. It follows, therefore, that if this hypothesis is to be compared with the modern theory, law is represented not by necessity, but chance, or τὸ οὗ ἕνεκα.

Again, comparison of hypotheses assumes some common basis, either of facts or method. But no such basis exists. The ancient speculation is purely subjective: it is an arbitrary assumption arrived at by no observation, and incapable of being tested by its application to facts. It is in reality a protest against the ignorance of facts which then hampered physical philosophy, an eager attempt to overleap the slow acquisition of knowledge, and "feel the future i' the instant." The one hypothesis is barren and lifeless: the power it invokes to bring order out of chaos is one by which order is impossible, since its operations at any time have no relation to the past, no influence on the future. The other is rich in results, for the law to which it appeals represents a continuous order of events, each of which is inseparably connected not only with the past and the future, but with all which takes place in the present. It is based on the slowly gained facts whose observation has occupied the 2,000 years since that other blind conjecture was jumped at. If the labours of those ages could be superseded by

such divination as is attributed to the predecessors of Aristotle, we would be driven back to what Lewes calls "the dangerous paradox," that science can exist without knowledge.

The present is in truth only another instance of that unphilosophical as well as unjust criticism which, struck with a verbal similarity, reads into vague guesses the precise statements of later thinkers, regardless alike of the context and of the contemporary history of the science to which these were necessary, but *in themselves* unproductive contributions.

But a more serious omission is one which raises justifiable doubts as to the competence of the critic to judge of scientific questions. The form of the reference to Darwin's views contains no allusion to geology, which supplies at once the most extended basis of facts for generalization, and in turn presents the widest field for the verification of the generalization, assuming always that those links in the proof which experiment and observation of living beings can complete have been so completed. The Greek had neither geology nor palæontology. He knew of alluvial formations, of upheavals and depressions of land, of fishes and shells found inland within the crust of the earth. But these were to him isolated facts. An eminent geologist, speaking of the writers of antiquity, says, "Standing altogether apart from fabled cosmogonies, there is dimly foreshadowed in their writings the *germ* of much that has of late years raised geology to the high rank it occupies among the sciences—I mean the close observation of existing phenomena of change, and their application to time past and future. Unrestricted by the dictates of a mistaken orthodoxy, they knew no limit to time, and thus, almost in the spirit of prophecy, the Stagyrite dared to assert that even as rivers and continents had heretofore sprung up and disappeared, so those that now are must also pass slowly away. The high acumen displayed by Strabo, in the application of the theory of upheaval to account for the occurrence of marine shells at a distance from the sea, is all the more wonderful when we consider that the inference was supported by the most slender portion of what now constitutes the mass of geological evidence. Knowing nothing of the absolute geological mechanism of the earth, yet deeming the laws of nature for ever unchangeable—marking well the present, and looking into the future—these men saw in the world an endless circle of mutability, which, in the language of Hutton, gave 'no vestige of a beginning—no trace of an end:' a marvellous inference by those who knew not that the events marking that wondrous history are recorded in tables of stone by the finger of Him who cannot lie."

The close observation of the Greeks is a frequent phrase—its proofs are less frequently put forward. Observation, as the term is used in modern science, is a very complex process, of which the mere use of the eyes is the lowest stage. The curious mind of the Greek accumulated numerous facts, which for the most part had to him nearly equal value. The unraveling of complex phenomena was, so far as the natural sciences are concerned, rarely attempted, Aristotle's great work on generation being perhaps the most signal exception. But in anatomy, where, of all others, observation should yield the most appreciable results, little that is accurate has come down to us. I have attempted in another place* to show that in the class Crustacea the anatomical structure of the animals has been treated with scanty care, and the instances might be multiplied by going through the treatises of other authors than Aristotle. The great majority of the facts correctly noted consists of those simple facts which are readily appreciable by sight; yet that even these are not always deemed of importance, every one is aware who has attempted to identify the animals or plants referred to by ancient writers. So far, therefore, from possessing materials necessary for speculations as to the changes which animals may have undergone, it is doubtful if even the characters of living species were well known—at least, none of the early naturalists have recorded any descriptions whence the animals referred to may be readily recognized.

How far, then, has their close observation been carried in geology? The alluvial phenomena of the Nile were the subject of shrewd comment by Herodotus, who applied what he learned in Egypt to the rivers of his own country. The evidence he adduces is excellent: the similarity of the off-shore sea bottom to the soil of the land, the existence of shells in the mountains, the saline efflorescence of the soil, the seaward projection of the delta, the difference of the soil of Egypt from that of adjacent lands. On such evidence he assumed the former existence of a Thessalian lake, of an estuary at Ilium, and the plains of Mæander. Thucydides also dwells on the power of the Achelous to connect the Echinades with the mainland by its alluvium. Many passages might be cited in which no indistinct allusions are made to the decay of land, and the filling up of sea by the sediment of rivers. The most distinct notices of the superficial change of the earth, and the causes of these changes—earthquakes, rains, and rivers—are contained in a treatise ascribed to Aristotle, but now believed by scholars to be a work of much later date—the treatise on the universe of Appuleius. But even including this authority (though for the

* *Ann. and Mag. of Nat. Hist.*, 1865. The Malacostraca of Aristotle.

present I would keep it out of consideration, as not bearing on the state of geological knowledge in the fourth century B.C.), what does it all amount to?

The remarkable passage in the *Meteorologia* (i. 14) has a rhetorical tone, a touch almost of enthusiasm, not common in the physical writings of Aristotle. In a subsequent chapter (ii. 3-5) he traces the circle of water from the sea to the mountain tops, and back to the sea. In this he describes the alterations of sea and dry land which the same region may undergo. As to the causes of these changes, I cannot adopt the view given by Schwarz, preferring the rendering of Barthlemy St. Hilaire, the enthusiastic translator of this work. The words of the text are,—"It is not right to consider the genetic development of the universe as the cause of this (alternation of sea and land); for it is ridiculous to say that the whole universe moves (literally to move the whole universe) by means of small and petty changes, and the mass and size of the earth is nothing in comparison with the whole heavens. But the cause of all these things must be understood to be, that there comes up, after fixed periods, a winter like the yearly one, a great winter of this great period, and an excess of rain; and this is not always at the same place," but is, like that of Deucalion, local. He instances several places which have at one time suffered from such floods. The occurrence of these changes is removed at once from the region of observation to that of *a priori* reasoning. "Now, since there is a necessity that the whole (universe) should undergo change, though neither genetic development nor decay, if the whole remains (or is eternal) it is necessity, we say, that the same parts should not always be covered by water (viz., sea and rivers) or dry land." The changes are slow in comparison with our lives—so slow that in the intervals the histories of nations are lost. Now, at first sight all this might be written to-day—so well does it represent modern science. But the recognition of these gentle changes is vitiated by one fatal omission—viz. the elevation and depression of land. The power of the sea and rivers is able to affect the surface of the country, but no more. To complete the work, periodical cataclysms are assumed as necessary. There is not a trace of a speculation as to stratified records of these changing waters; nay, the contrary may be assumed from the denial of the genetic development of the earth; for the slow addition of stratum to stratum would doubtless have presented itself as the stages of such development. Even the terminology did not exist whereby strata could be described: these were either invented at a much later date, or words were used in a special, technical way (*e. g.*, ἔδαφος).

Xenophanes, of Colophon, is reported by Origen (in the second

century, nearly 700 years, therefore, after the founder of the Eleatic school) as conjecturing the types of fishes and phokes found in the depths of the rock at Syracuse and elsewhere to have been called into existence when all things were in a state of mud, and to have been thereafter desiccated.* But this opinion is connected with a cataclysmal theory which, like that of most of the philosophers of his period, had its source rather in morals than physics. Periodic cosmical destructions were, says Origen, assumed by Xenophanes: by these mankind were removed from the earth, and the genetic process commenced anew. The vehement protests against the luxury of his times which are founded in *The Fragments of Xenophanes* render these cataclysms rather depuratory than geological. This mixture of physics and metaphysics is preserved in the derived Roman Philosophy:—"Quandoque erit terminus humanis rebus quum partes ejus interire debuerint, abolerive funditus totæ, ut de integro totæ rudes innoxiæque generentur, nec supersit in deteriora magister." The idea of a periodic destruction, as here put forward, is only in form like that of Xenophanes and other early Greeks, who assigned it as a cosmical phenomenon; whereas Seneca found it inherent in the earth itself. Still the moral ground at both ages is the same—the philosopher's conviction that evil cannot last for ever—in fact, a form of theological teleology.†

The Ionic School furnishes nothing of sufficient precision, as to the operations of nature, to collate with the tenets already mentioned. Their cosmogonies were too purely metaphysical, as their country presents nothing salient in geological structure to draw forcibly attention to it.

Far different was it with Empedocles. Born in a country where the terrible effects of volcanic action were ever present to the minds of men, and had given a character to their traditions, which, linking as it did the terrible convulsions they witnessed with tales of the power or vengeance of the gods, it was inevitable that his doctrines should be influenced by the phenomena around—phenomena which forced themselves on the attention far otherwise than did the tame Ionian scenery on that of Thales and his successors. Xenophanes and Aristotle

* These statements, quoted by Schwarz, are not introduced into Mullachius' edition of *The Fragments of Xenophanes.* Paris, 1864.

† This was, moreover, a part of the gains of Greek from foreign philosophy. The degeneracy of the race was, among the Egyptians, an undisputed belief: the removal of the degenerate by means of cataclysms, to make way for a better race, was part of their order of nature, subordinate, however, to the grander epochs of the Great Year. This subordination was lost sight of in the transfer, and the cataclysms appear as of greater or less importance in the tenets of different schools of philosophy.

dreamed of subterranean reservoirs of water. Empedocles, born in a volcanic country, and conversant with the Pythagorean doctrines, familiar, too, with the social and political unrest of a region whose inhabitants seem at all times to have been impelled by strong passions, whose enthusiasm even has a tinge of fierceness in it, seems to have been more than usually influenced by the external conditions under which he lived. The struggles of nature he looked on as the counterpart of the human conflicts about him—neither seemingly under the control of any regulating law—both equally short-lived and recurrent. Nor were apparent confirmations of this doctrine far to seek: the tertiaries of Sicily supplied fragments * in which the cosmogonists found proofs of the earlier monstrosities to which chance gave rise during the progress of all things from chaos, data whence Ovid probably drew his different view, and constructed his inverted sequence of the age.

The geological speculations of Empedocles are so interwoven with what has been called his palæontology, that they cannot be spoken of separately. I shall, therefore, first speak of the fossils known to Anaximander, which are the better entitled to consideration that

* Though not bearing directly on my argument, I would draw attention to the additional instance here furnished of the local character of several movements in geology; or, in better terms, of the impress which these movements have received from the localities in which they originated. The tame Ionic scenery left its sages free to ponder over human thoughts and actions, and to sink into that verbal analysis of which some part of their philosophy consists, undisturbed by objective terrestrial phenomena. To Aristotle, the traveller, convulsions of the earth were too slightly known to figure prominently in his system; water was a more familiar agent, of which subterranean reservoirs usurped the place assigned by the Pythagoreans to fire. That sect, whose stronghold may be said to have been Southern Italy, had this particular tenet confirmed at least, if not actually suggested by the land in which their order flourished, as the Chaldæan astronomy was nursed on the open plains whence the heavens could so advantageously be surveyed. Empedocles came under the same influence; and both he and the Pythagoreans found in the strata beside them the basis of their respective speculations as to the course of previous ages. Coming nearer to our own time, Neptunism sprang into a creed in a limited district whose very igneous rocks had somewhat of the aspect of sedimentary strata. And when, at the beginning of this century, geology assumed much of the character and most of the tendencies it now shows, these were impressed on it by Cuvier, as the inevitable consequence of his explorations in the Paris basin. For the geology of to-day is but slowly resuming the character of a physical study which it had before Cuvier gave fossils so prominent a place, and before the London Geological Society, under the inspiration derived from him, accumulated its observations in a country as rich as France in well-preserved fossils.

Cuvier has made them the occasion of a criticism amply illustrative, as Sir C. Lyell has pointed out, of the kind of assertion against which this paper is a protest. Lyell has pointed out the rashness with which the French naturalist, doubtless not having consulted the original texts, found a progressive transmutation comparable to that of Lamarck, in the reports of Anaximander's doctrines, given by Plutarch, Eusebius, and Censorinus. The sum of these fragments is, that the first animals were generated in slime by the action of the sun, were covered with prickly integuments, which being thrown off, the animals speedily died: that men came into being in fish, since, as they are at present constituted, the human young are incapable of self-support, but that after a time, when they were capable of caring for themselves, they assumed their proper shape. Such is the evidence given in the authorities cited by Lyell; but Origen, *Philosophum*, p. 11, ed. Miller, slightly modifies the statements; for he says that "animals came into being under the evaporating power of the sun; and that man came into being in another animal—one similar to a fish—in the beginning." If this passage is a correct account of the facts, the gap between Anaximander's primordial animals and man is very small, or at least it is not necessary to believe that any long series intervened; the speculation is therefore distinct from that of Empedocles. In commenting on it, Schwarz ingeniously suggests the possibility of the philosopher having seen for himself the remains of fishes, perhaps of Devonian fishes, taken out of the solid rock. He gives no reference, however, to any modern notice of the Devonian rocks of Asia Minor having yielded *coccosteus* or *cephalaspis*, to which he seems to allude. Moreover, he goes, it seems, too far, if the passage from Origen be correct, in assigning fish as the progenitors of the human race. Origen says the forms were fish-like; and the words of Plutarch, *De Placit. Phil.*, v. 19, quoted by Schwarz himself (about the prickly integuments which were shed), indicate a belief in temporary investments, not necessarily in forms of such permanence as require transmutation to give rise to the higher creation. We may, in fact, compare his notion with what is known of insect metamorphosis: the chrysalis in its case might be compared to a worm, whence the insect issues, an animal of a higher type, which, under the figure of a lower, passed that period in which it was incapable of procuring its own nourishment in the same way as the adult form. Be this as it may, the Milesian held, as did all who had been pupils of the Memphian priests, that all things returned again to their primitive elements.

Empedocles, too, was aware of the existence of fossil remains; but these seem to have been of a different kind from those which Anaxi-

mander had seen—different, too, from those on which Herodotus speculated, under the guidance of Egyptian observers. Those, at least, which have most distinctly impressed his mind, figuring prominently in his poems, are such as might, according to Schwarz's suggestion, have been found in the Sicilian caverns. It is with hesitation that I venture to differ from an author like Schwarz, who, to a general knowledge of the history of geology, joins an enviable familiarity with ancient languages—who, as I am told by one of his countrymen, is more familiar with Greek than some of us are with our own tongue. But I think his enthusiasm in the cause of Empedocles has carried him somewhat too far. He compares the Acragantinian's remarks upon these huge fossils to the reconstructions of a Cuvier, a Wagner, or an Owen. His work, remarkable for the knowledge of Greek authors which it displays, is unfortunately but little known in this country.*

He has adopted the arrangement of Sturtz; and this, I think, has given rise to the opinions which it is my purpose to controvert. The arrangement of Mullachius † seems to me to offer a more coherent whole; but this is purely a matter of opinion, and requires a skill not less than that of Cuvier to re-arrange the scattered mixed fragments of the truly great philosopher.

Empedocles believed in the fortuitous concourse of atoms. His two principles—attraction and repulsion, discord and love, or whatever other names may be chosen—are those on which the chaos or order of the world depend. Superior to these two is necessity, the eternal will of the gods; but the part played by this power is as obscure, though less distinctly stated, than the "mind" of his contemporary, Anaxagoras. He also seems to have recognized another set of divine powers or principles, which, however, take small share in his physical speculations. In geology he has clear volcanic notions, shares with the Pythagoreans a central fire and subterranean streams of fire, and asserts the elevatory power of that fire; but it is merely telluric, and has not the cosmical value assigned to it by Pythagoras and his school.

It is, I think, clear that the two principles of attraction and repulsion form a somewhat quaquaversal proposition, ‡ both alike being at times the causes of order and chaos. But the general law is also clear, that the domination of either power gives rise to a period in which the condition peculiar to each prevails. The course of events is summed up in pseudo-Plutarch. Empedocles says, "that the first origins of

* *On the Failure of Geological Attempts in Greece.* By Julius Schwarz, London, 1862.

† *Fragm. Phil. Græc.* recensi ut Fr. Aug. Mullachius. Paris, 1864.

‡ Sturz, *Comment. de Empedoclis vita et Philosophia*, pp. 225 *et seq.*

plants and animals were by no means perfect, but scattered in disunited parts; the second stage was the union of similar parts; the third, of those parts which are developed from each other; the fourth origin is no longer from similar parts, as from earth and water, but they (the beings) are generated by each other by those which, on the one hand, have digested food, and, on the other, the symmetry of woman has impressed the stimulus of the spermatic movement. Schwarz finds in the remains of Empedocles the stages of this series; but, oddly enough, the differences of text which enable him to find them, and have made me fail, are noted by neither editor. The form which Mullachius gives his text is certainly confirmed by the order stated in Lucretius, whose authority, admitted in other cases, has the advantage of being nearer by centuries to the period of Empedocles. I am inclined to believe that the lines which describe the joint influence of Hephaistos and Aphrodite in the production of the components of a body, are embryological, since it and the passages concerning the different qualities of the male and female are in keeping with the Aristotelian phraseology, if not doctrine. As the text now stands, it seems to be hypercritical imagination to find a parallelism in it to the pseudo-Plutarchian sketch. Johannes Philoponus (*ad Aristot. De Gen.*, An. i. 18) is cited as stating that, at the close of the chaotic period, all these disjecta membra, "endowed with vital principle and with sensation or sensitiveness, *were in the earth, and from these, as from many animals, were generated each of the animals.*" Here Aristotle's evidence is of value, as given in the passage quoted at the beginning of this paper.

Moreover, as these scattered parts preceded in age the *βουγενῆ ἀνδρόπρωρα*, and as, by the pseudo-Plutarch's own showing, reproductive power was wanting to them, the whole case falls to the ground.

Palæontology there is none. The Greek saw clearly the facts, and a remarkable account of them it is; but the sequence was beyond his power. The eternal circle of matter—the periodic revolutions—the start afresh of all things—this is all we find. The power of volancoes is clearly known—the reproductive power of water not so understood as it was by the Ionic school. Nay, the classification given by Sturz as an epitome of Empedocles—that, namely, by which animals are divided into terrestrial and aquatic, according to the dominance of one or other primitive element—puts an end to all possibility of coherent succession of life. Here Empedocles gives us, as in his scheme of the world, parts which may indeed form a whole, but, like them, the inspiration had not yet come.

Aristotle was aware of the existence of fossils. Fossil fish he men-

tions as living in the ground motionless, and as being dug out of the ground. Their existence he connects with their colder habit and less urgent need of respiration. He regarded them, therefore, as parts of the present order of things, and thus the passage in the *Meteorologia* already quoted, can be merely a speculation in physical geography.

As an illustration of the thesis already put forward concerning the local character of the ancient cosmogonic theories, the doctrine of Anaxagoras may be mentioned. He, too, was a native of Asia Minor: his opinions therefore lacked the Sicilian stamp. Earthquakes were to him the result of subterranean air. But, like his contemporary, Empedocles, he taught the eternity of matter, and put into a clear formula that the origin of things was a mixture, their destruction a resolution. The *homoiomeræ* of which they consisted—similar particles, whose combination went to make up particular bodies—existed as such in a chaotic state,—were potentially the elements of future bodies, but received the impulse to combination from without, from an all-pervading Νοῦς, or Intelligence. The commencement of this era of intelligence was that of the present order of things seemingly—an order which has not yet run its course. But though there is no formal intimation of any periodic changes, such as Empedocles believed in, it seems as if there were a reserve in the fragments of this author—a tacit admission that the resolution of the *homoiomeræ* might again come. The fuller possession of his poems might explain this point. But be this as it may, it is evident that as a whole the uniformity which Anaxagoras asserted is inferior to that of Empedocles; for while the latter accepted catastrophes as a part of the order of things, Anaxagoras seems unaware of their import. In one passage he speaks of stones as growing by a creation of earth, but adds, that they commonly come out of water—a far off hint of aqueous deposit; yet, vague as it is, it is nearer stratification than any words of Empedocles.

Xanthus, the Lydian, who flourished about 480 B.C., is cited as possessing knowledge of fossils; but clearly as he speaks of them, his notion that their inland position is the result of a gradual dessication of the sea removes him from the class of anticipators of modern geology.

It is difficult to deal with the Pythagoreans. Of the Pythagoras whom Maurice thinks it the most probable among guesses to assign as the pupil of Anaximander, who returned from Egypt and Persia rich in knowledge gained from the philosophers of these countries, and established in Magna Græcia "orders," as Thirlwall calls them, which must have exercised great influence on the social state of that region,—of the Pythagoras to whom Aristotle and Plato yielded a

respect in which his followers did not share, nothing remains whence his physical doctrines may be fairly judged. Accounts of his teachings there are; but these are, on the one hand, preserved by men who had not been in the strictest sense his pupils, or by Neo-platonists, in whose writings the old philosophy appears shrouded in a Christian veil. Sir C. Lyell has taken the account given by Ovid of Pythagorean philosophy as representing the then beliefs of the sect in the Augustan age; and it is fortunate that such an account of the physical creed of the school has been preserved at a period prior to that of the wider influence of Christianity, whose similarity to the ancient faith, in its insistence on the divine relations of human society, might have led at a later date to the attribution to the heathen system of part at least of the Hebrew cosmogony. The fragment concerning nature attributed to Ocellus Lucan opens with the assertion of the eternity of the world, and of the absence in it of any sign either of beginning or end—words almost identical with those used by Playfair. The commentary of Hierocles, on the *Aureum Carmen*, contains similar, though less distinct, phrases; but the great bulk of that work is a strange mixture of heathen and Christian theology and morality. The insensible something that the changed state of opinion superadded may be readily felt, though it scarcely admits of definition, by the perusal of Hierocles' lengthy disquisitions, and their comparison with the terser fragments of the heathen Archytas the Tarentine.

The summary in the *Principles of Geology*, i., 17, of the passage in Ovid (*Metamorph.*, xv. 165, *et seq.*), is as follows :—

"'Nothing perishes in this world, but things merely vary and change their form (l. 165). To be born means simply that a thing begins to be something different from what it was before; and dying is ceasing to be the same thing (l. 215). Yet, although nothing retains long the same image, the sum of the whole remains constant' (l. 254).

"These general propositions are then confirmed by a series of examples—all derived from natural appearances, except the first, which refers to the golden age giving place to the age of iron. The illustrations are thus consecutively adduced—

"1. Solid land has been converted into sea (l. 262).

"2. Sea has been changed into land, marine shells lie far distant from the deep, and the anchor has been found on the summit of hills.

"3. Valleys have been excavated by running waters, and floods have washed down hills into the sea (l. 267).

"4. Marshes have become dry ground (l. 268).

"5. Dry lands have been changed into stagnant pools (l. 269).

"6. During earthquakes some springs have been closed up, and new ones have broken out; rivers have deserted their channels, and have been reborn elsewhere, as the Erasenus in Greece and Mysis in Asia (l. 270-80).

"7. The waters of some rivers, formerly sweet, have become bitter, as those of the Anigros in Greece, &c. (I. 280).

"8. Islands have become connected with the mainland by the growth of deltas and new deposits, as Antissa and Pharos, &c.

"9. Peninsulas have been divided from the mainland, and have become islands.

"10. Land has been submerged by earthquakes.

"11. Plains have been upheaved into hills by the confined air seeking vent.

"12. The temperature of some springs varies at different periods.

"13. There are streams which have a petrifying power.

"14. Some rocks and islands, after floating, have become stationary and immovable.

"16. Volcanic vents shift their position."

The opening sentences of this extract are identical with those of Ocellus. Its whole tenor, greatly extended in comparison with the Aristotelian exposition of superficial changes, and distinguished from the views of Empedocles by the subordination of volcanic or elevatory forces in the courses of things which thus go on without catastrophic interruptions, shares, nevertheless, the censure justly passed by Lyell on all Greek science—"that it never compared attentively the results of the destroying and reproductive operations of modern times with those of remote eras, nor had it ever entertained so much as a conjecture concerning the comparative antiquity of the human race, or of living species of animals and plants, with those belonging to former conditions of the organic world. The Greek philosophers had studied the movements and positions of the heavenly bodies with laborious industry, and made some progress in investigating the animal, mineral, and vegetable kingdoms. But the ancient history of the globe was to them a sealed book; and although written in characters of the most striking and imposing kind, they were unconscious even of its existence."

From this censure not even Strabo is exempt; for though he, as Lyell points out, asserts the safety-valve view of volcanoes in anticipation of modern theory—an anticipation, by the way, purely conjectural—and though he lays down the axiom, that explanations of these phenomena are to be sought in things which are obvious, and in some measure of daily occurrence, as deluges, earthquakes, and volcanic eruptions, yet no linking of the past, of the remote past, with the present, is attempted.

Enough has been said to prove that, at the time of Empedocles, no theories existed in any way comparable with those of modern geology—that the knowledge on which alone such theories could be built had not been acquired. Much was known, but only such phenomena as were obvious; and though the reasoning from these obvious facts has a

specious resemblance to geological speculation, yet, on inquiry, the resemblance fades into vague guesses, as isolated as the facts whence they proceeded. If it is asked why these guesses remained so long barren and unproductive, the answer must be sought partly in the purpose with which they were advanced, in the nature of the systems of which they formed a portion—partly in the character of the people themselves.

All the Greek speculations were purely cosmogonic: of an earth without man, they seem to have had no conception; for even in Empedocles the indications are faint of any such hypothesis having been ingrafted on the Greek mind by its Oriental teachers. In consequence, the earth as it is was to them the earth as it had been: catastrophes might come, but they were either local or subversive wholely of all that existed. A portion of mankind was destroyed, or if the whole perished, another race of men appeared. Such changes were effected for moral purposes, and altered only the surface. That these alterations left their records in the earth's crust was never dreamt of.

Again, the chain on which these beads of speculation were hung was not one of physical but of metaphysical reasoning. They were more or less, but all to some extent, and that the greater, the happy illustrations seized by active, fertile imaginations, for the purpose of bearing out their views of man, and his conditions and relations—not studied by themselves and for themselves. This brings us to the third source of explanation—the genius of the people among whom these theories sprung up. Schwarz sums up their character in pithy words of grotesque force:—"All the five senses appear in them highly developed. Perhaps this exalted them in a manner so unfavourable to the modern direction of science—they were conducted as positively in their philosophy by their auditory organ as by their extremely fine nose or fondled gustatory nerve: perhaps this caused, as to the actual configuration of things on the surface of our planet, their utter indifferentism—that their imagination, frantic as it was, rendered their most cursory conceits, their most notable lies, a benumbed group of statues, a life-long object of their adoration. Preferably, sculptors, painters, musicians, apt to perform at religious gazing as on a seven-chorded lyre,—lawyers, physicians, foot-soldiers, seamen, cooks, jesters, gymnasts, coachmen on trijugal, quadrijugal cars,—subtle, ambitious, perfidious, quarrelsome, never so prone to curiosity as to pertness in their conclusions,—always more acute than profound, thus fulfilled the Greeks their political duty, and such is their fame, transmitted to posterity on every stone they cut, on every tablet they inscribed."

Part only of this is true, and that part only as regards physical observation. In astronomy, Lewis maintains their deficiency in exactness; and Dr. T. Young expresses a natural wonder that men of such great talents and varied ingenuity should never have thought of subjecting their conclusions in physics to the test of experiment.

This seems the real cause of the Greek failure in physical inquiries. Their impulses in that field of inquiry seem to have been almost exclusively derived from without; and the chilling influences which reduced that impulse to nothing—which reduced natural science to a marginal commentary on other philosophies—must be sought in their political condition. Each state had a government which it believed the best, and for which it was ready to fight to the death. In small states the segregation of a class of thinkers, apart from the actual politically interested workers, is impossible. By nature and circumstances compelled to politics, the exceptions are few in which the philosopher was apart from the theorist in state craft; and these few exceptions, as notably Anaxagoras, were given up to verbal puzzles. Whatever bore as an illustration on the principles of government, or primarily on the constitution of man's moral and intellectual being, was adopted, elucidated by metaphysics, passed into the system of the adopter, and ceased thenceforth to form a part of a distinct field of inquiry.

I propose in a future communication to enter upon the zoological anticipations of the ancients, and to prove that there, also, the Greeks were as incompetent, from defect of knowledge, to construct a theory of the origin of species, as I believe myself to have proved that they were from a geological point of view.

Since the above was written, I have found in a volume of occasional criticisms (privately printed a few years ago), by my colleague, Professor John Nichol, a notice of Empedocles. It is unfortunately only a sketch, but is, like the other philosophical critiques in the same volume, a clear and just summary of a very complicated subject. Empedocles is treated of at greater length, but neither so clearly nor so reliably, in a recent number of the *North British Review*. That article is rather a popular paraphrase of the salient features of the philosophy, while, strangely enough, the physical portions of Empedocles' speculations are but lightly touched on.

IV.—*On the Proposed Extension of the Factory Act, and its Probable Effects on Juvenile Labour and Education in Glasgow.* By JOHN D. CAMPBELL, ESQ.

Read January 23, 1867.

MR. CAMPBELL began by sketching the leading provisions of the Act for restricting the hours of labour and promoting the education of children and young persons, the former being defined to mean persons under thirteen years of age, and the latter persons between the ages of thirteen and eighteen. Children must be employed as half-timers, the rule being that no child employed before twelve can be employed after one, so that the maximum time for which a child can be employed is seven and a half hours—*i. e.*, from six to one, with half an hour for breakfast. In cases, however, where all women and young persons are only employed ten hours, instead of ten and a half, children may be employed for full time on alternate days, instead of half-time every day. Those children who are employed for half the day go to school on the other half for three hours daily, and those who are employed on alternate days go to school for five hours on the days when they are not employed. The half-time system prevailed much more extensively in England than in Scotland, particularly in the manufacturing districts of Lancashire. In Johnstone—where there were a good many cotton and flax mills—and other places, a considerable number of half-time children were employed; but in Glasgow things were in a different position. Here a great many works do not come under the Act, and children can find employment there without restriction, so that manufacturers prefer to employ persons above the age of thirteen, rather than submit to the inconveniences of the half-time system; while parents, as soon as practicable, were found to withdraw their children from the factories, and place them at other work to which the Act had not extended, and where they might earn double wages. A circumstance, also, which tended to make the half-time system more difficult to work in Scotland than in England was the difference in the dinner hour, which in Scotland was two o'clock, while in England it was sometimes twelve and rarely later than one—thus affording a more suitable division of the day, one staff of children working up till that hour, and being then replaced by another. The works which were under the Factory Act ten years ago might be defined as all places where spinning and weaving of textile fabrics were carried on, dyeworks

and potteries not being included. Printworks were in rather a peculiar position. They were not under the Factory Act, but under the Printworks Act, which was very inefficient and unsatisfactory, and would be better repealed altogether, as it made most inadequate provision for education, while children of eight years might be employed from six in the morning till ten o'clock at night without the Act being infringed.

A Children's Employment Commission had made a minute investigation of the position of the various trades, and from time to time they had issued reports on the subject—five in all—on some of the early ones of which action had been taken. The Factory Act had thus already been considerably extended, so that, besides spinning and weaving mills, it now applied to employment in bleaching, dyeing, and finishing works—in potteries, lucifer-match works, in paper-staining works, and several others which were not extensively carried on, such as cartridge-making. Its introduction to bleachworks and potteries had been attended with most important benefits, and had proved the greatest blessing to the former—not particularly with reference to children, but to women, whose position had been about the worst of that of any trade in the country, not from the fault of the employers, but from the system—which scarcely any individual employer could afford to break through—of being obliged, within a limited time, to execute orders in a hurry, which they must have either performed or lost—and probably lost with them also the custom of the merchant who gave them. The effect on potteries and other works which had been brought under the Act had been to increase the number of half-timers. The potteries employed a large number of young children. In recent reports of the Children's Employment Commission, it was recommended that almost all trades which employed a considerable number of women and children should be brought under the Factory Act—such as printers, metal trades, bookbinders and stationers, tobacco spinners, packing-box makers, &c., making, in some instances, provision by which night-work should be permitted. If the Act were thus extended, great difficulty would be found in works in Glasgow, where juvenile labour was extensively employed, by their having to adopt the half-time system. If this system was to be carried on, it would be necessary that special schools, or, at all events, separate instruction, should be provided for the children. It had been recommended by Sir John Kincaid, that in place of the half-time system there should be an educational test required in order to employment; and that in the case of those who could read or write, they might be employed as young persons at the age of twelve years, instead of thirteen—while those who

could not read and write should not be admitted as such till the age of fourteen. Those interested in education should take these questions into consideration. He thought that no one who observed the general feeling of the country and the action of Ministers, could doubt that before long some Act would be passed, which, if not carrying out all the recommendations of the Children's Employment Commission, would at least do so to the extent of making a material difference in the employment of children in Glasgow.

A conversation followed Mr. Campbell's address, in the course of which

MR. GEORGE ANDERSON, after referring to the lucid explanation of the Act which had been given, said he thought there was a great deal to be said both in favour of the Act and the extension of it. As regards the protection it afforded to children and females, it had been in every way satisfactory. Looked at, however, in an educational point of view, he could not say so much; and as regards the position of employers, immense injustice had been suffered by those under the Act as compared with those in other trades, and they had not had fair competition in the labour market, particularly in Glasgow, where the variety of employment is so great. The Children's Employment Commission seemed to think that those trades at which large masses of children were employed should be brought under the Act. His idea was, that there ought to be a general law as regards the protection of women and children, whether employed in one kind of factory or another, and in place of having one trade with special provisions and special inspection, the first step ought to be a general law. In reference to education, one of the misfortunes connected with the Factory Act was, that the education given under the half-time system was entirely illusory. The working classes never could become educated under it; the Factory Act in that respect would never do in the world; and they must have something more comprehensive if they were ever to educate the people. It was a most disgraceful state of matters that millions of children were growing up in this country without education, and still more disgraceful, perhaps, to the clergy than to others, because it was by them that nearly every attempt at education made in the country had been baffled. Rather than concede any of their small sectarian differences, they would allow the whole country to remain uneducated. Public opinion was growing in favour of compulsory education. He was strongly convinced that an educational test would do a great deal more towards the education of children than the half-time system; but at the same time he looked upon it only as an expedient. It was only a half measure, and he would willingly go the whole length of a compulsory measure of education.

Mr. George M'Callum stated that children who did not come to the factories till they were thirteen were sent to other trades, and their education entirely neglected. He was strongly of opinion that an educational test was perhaps the best thing that could be adopted either in the case of works to which the Act at present applied, or those trades to which it might be extended.

The Rev. Mr. Crosskey said he agreed that there must be a comprehensive and full system of education, and that there was no conceivable means of meeting the necessities of the country except by something universal and compulsory. As to the blame thrown on the clergy in relation to this subject, he was inclined to think that the fault lay entirely with the laity. If the laymen of Scotland would take up the education question—if manufacturers and those interested in commercial pursuits would have some regard for the mental progress of the community, and be really determined to settle this question by obtaining a comprehensive measure of education of fair and noble proportions, then not all the clergy of Scotland would stop the will of the united laity of the country.

Mr. Campbell made a few remarks in reply, and expressed the opinion that if the half-time system were fairly carried out at the schools to which the children went, a tolerable education might be acquired. At the same time, it was not the best possible system.

V.—*On the Phrase "Potential Energy," and on the Definitions of Physical Quantities.* By W. J. Macquorn Rankine, C.E., LL.D., F.R.SS. Lond. and Edin., &c.

Read January 23, 1867.

1. In the course of an essay by Sir John Herschel "On the Origin of Force," which appeared some time ago in the *Fortnightly Review*, and has lately been re-published in a volume entitled *Familiar Lectures on Scientific Subjects*, the opinion is expressed that the phrase "Potential Energy" is "unfortunate, inasmuch as it goes to substitute a truism for the announcement of a great dynamical fact" (*Familar Lectures*, page 469.)

2. There is here no question as to the reality of the class of relations amongst bodies to which that phrase is applied, nor as to any matter of fact concerning those relations, but as to the convenient and appropriate use of language. This is a sort of question in the discussion of

which authority has much weight; and when an objection to the appropriateness of a term is made by an author who is not less eminent as a philosopher than as a man of science, and whose skill in the art of expressing scientific truth in clear language is almost unparalleled, it becomes the duty of those who use that term to examine carefully their grounds for doing so.

3. As the phrase "Potential Energy," now so generally used by writers on physical subjects, was first proposed by myself in a paper "On the General Law of the Transformation of Energy,"* read to the Philosophical Society of Glasgow, on the 5th of January, 1853, I feel that the remark of Sir John Herschel makes it incumbent upon me to explain the reasons which led me, after much consideration, to adopt that phrase for the purpose of denoting all those relations amongst bodies, or the parts of bodies, which consist in a power of doing work dependent on mutual configurations.

4. The kind of quantity now in question forms part of the subject of the thirty-ninth proposition of Newton's *Principia;* but it is there represented by the area of a figure, or by symbols only, and not designated by a name; and such is also the case in many subsequent mathematical writings.

5. The application of the word "force" to that kind of quantity is open to the objection, that when "force" is taken in the sense in which Newton defines "vis motrix," the power of performing work is not simply force, but force multiplied by space. To make such an application of the word "force," therefore, would have been to designate a product by the name properly belonging to one of its factors, and would have added to the confusion which has already arisen from the ambiguous employment of that word.

6. The word "power," though at first sight it might seem very appropriate, was already used in mechanics in at least three different senses: viz.—*first,* the power of an engine, meaning the rate at which it performs work, and being the product of force and space divided by time; *secondly,* the power, in the sense of effort or pressure, which drives a machine; and *thirdly,* "mechanical powers," meaning certain elementary machines. Thus "power" was open to the same sort of objection with "force."

7. About the beginning of the present century, the word "energy" had been substituted by Dr. Thomas Young for "vis viva," to denote the capacity for performing work due to velocity; and the application of

* Viz.,—that the effect of the presence of a quantity of actual energy, in causing transformation of energy between the actual and the potential forms, is the sum of the effects of all the parts of that quantity.

the same word had at a more recent time been extended by Sir William Thomson to capacity of any sort for performing work. There can be no doubt that the word "energy" is specially suited for that purpose; for not only does the meaning to be expressed harmonize perfectly with the etymology of *ενέργεια*, but the word "energy" has never been used in precise scientific writings in a different sense; and thus the risk of ambiguity is avoided.

8. It appeared to me, therefore, that what remained to be done was to qualify the noun "energy" by appropriate adjectives, so as to distinguish between energy of activity and energy of configuration. The well-known pair of antithetical adjectives, "actual" and "potential," seemed exactly suited for that purpose; and I accordingly proposed the phrases "actual energy" and "potential energy," in the paper to which I have referred.

9. I was encouraged to persevere in the use of those phrases, by the fact of their being immediately approved of and adopted by Sir William Thomson; a fact to which I am disposed to ascribe in a great measure the rapid extension of their use in the course of a period so short in the history of science as fourteen years.* I had also the satisfaction of receiving a very strong expression of approval from the late Professor Baden Powell.

10. Until some years afterwards I was not aware of the fact, that the idea of a phrase equivalent to "potential energy" in its purely mechanical sense had been anticipated by Carnot, who, in an essay on machines in general, employed the term "force vive virtuelle," of which "potential energy" might be supposed to be almost a literal translation. That coincidence shows how naturally the phrase "potential energy," or something equivalent, occurs to one in search of words appropriate to denote that power of performing work which is due to configuration and not to activity.

11. Having explained the reasons which led me to propose the use of the phrase "potential energy," I have next to make some observations on the objection made by Sir John Herschel to that phrase, that "it goes to substitute a truism for a great dynamical fact."

12. It must be admitted that the use of the term "potential energy" tends to make the statement of the law of the conservation of energy wear to a certain extent the appearance of a truism. It seems to me, however, that such must always be the effect of denoting physical relations by words that are specially adapted to express the properties of those relations; or, what amounts virtually to the same thing, of

* Sir William Thomson and Professor Tait have lately substituted the word "kinetic" for "actual."

drawing up precise and complete definitions of physical terms. Let A and B denote certain conceivable relations, and let them be precisely and completely defined; then from the definitions follows the proposition, that A and B are related to each other in a certain way; and that proposition wears the appearance of a truism, and is virtually comprehended in the definitions. But it is not a bare truism; for when with the definitions are conjoined the two facts, ascertained by experiment and observation, that there are relations amongst real bodies corresponding to the definition of A, and that there also are relations amongst real bodies corresponding to the definition of B, the proposition as to relation between A and B becomes not a bare truism, but a physical fact. In the present case, for example, "actual energy" and "potential energy" are defined in such a way as to make the proposition, That what a body or a system of bodies gains in one form of energy through mutual actions, it loses in the other form—in other words, that the sum of actual and potential energies is "conserved"—follow from the definitions, so as to sound like a truism; but when it is proved by experiment and observation that there are relations amongst real bodies agreeing with the definitions of "actual energy" and "potential energy," that which otherwise would be a truism becomes a fact.

13. A definition cannot be true or false; for it makes no assertion, but says, "let such a word or phrase be used in such a sense;" but it may be *real* or *fantastic*, according as the description contained in it corresponds or not to real objects and phenomena; and when, by the aid of experiment and observation, a set of definitions have been framed which possess reality, precision, and completeness, the investing of a physical fact with the appearance of a truism is often an unavoidable consequence of the use of the terms so defined.

14. In the case of physical *quantities* in particular, the definition involves a rule for measuring the quantity; and the proof of the reality of the definition is the fact, that the application of the rule to the same quantity under different circumstances gives consistent results, which it would not do if the definition were fantastic; and hence the definitions of a set of physical quantities necessarily involve mathematical relations amongst those quantities, which, when expressed as propositions and compared with the definitions, wear the appearance of truisms, and are at the same time statements of fact.

15. In illustration of the foregoing principles, it may be pointed out that there is a certain set of definitions of the measurement of time, force, and mass, which reduce the laws of motion to the form of truisms, thus—

I. Let "*Equal times*" mean the times in which a moving body, under the influence of no force, describes equal spaces. This definition is proved to be real by the fact, that times which are equal when compared by means of the free motion of one body, are equal when compared by means of the free motion of any other body. If the definition were fantastic, times might be equal as measured by the free motion of one body, and unequal as measured by that of another.

II. Let "*force*" mean a relation between a pair of bodies such that their relative velocity changes, or tends to change, in magnitude or direction, or both; and let "*equal forces*" mean those which act when equal changes of the relative velocity of a given pair of bodies occur in equal times. This definition is proved to be real by the fact, that the comparative measurements of forces made in different intervals of time are consistent with each other, which would not be the case if the definition were fantastic.

III. Let the "*mass*" of a body mean a quantity inversely proportional to the change of velocity impressed on that body in a given time by a given force. This definition is proved to be real by the fact, that the ratio of the masses of two given bodies is found experimentally to be always the same, when those masses are compared by means of the velocities impressed on them by different forces, and in different times; and is also the same, whether each of the masses is measured as a whole, or as the sum of a set of parts.

Assuming those definitions as merely verbal, without reference to their reality, the laws of motion take the form of verbal truisms; but when experiment and observation inform us that permanent relations exist amongst real bodies and real events corresponding to the definitions, those apparent truisms become statements of fact.

16. One of the chief objects of mathematical physics is to ascertain, by the help of experiment and observation, what physical quantities or functions are "conserved." Such quantities or functions are, for example—

I. The *mass* of every particle of matter, conserved at all times and under all circumstances.

II. The *resultant momentum* of a body, or system of bodies, conserved so long as internal forces act alone.

III. The *resultant angular momentum* of a body, or system of bodies, conserved so long as internal forces act alone.

IV. The *total energy* of a body, or system of bodies, conserved so long as internal forces act alone.

V. The *thermodynamic function*, conserved in a body while it neither receives nor gives out heat.

In defining such physical quantities as those, it is almost if not quite impossible to avoid making the definition imply the property of conservation; so that when the fact of conservation is stated, it has the form of a truism.

17. In conclusion, it appears to me that the making of a physical law wear the appearance of a truism, so far from being a ground of objection to the definition of a physical term, is rather a proof that such definition has been framed in strict accordance with reality.

VI.—*Notice of Two New Instruments.* By JOHN YOUNG, M.D., F.R.S.E., Professor of Natural History in the University of Glasgow.

Read February 20, 1867.

THE definition of "species" and "variety," ever a fruitful source of discussion in Natural History, is scarcely nearer precise settlement than it was some years ago. For, as the number of known animals increases, and as the localities where the same genus occurs, are multiplied, intermediate forms are multiplied in the same proportion. But since the publication of Mr. Darwin's views, it has become of the utmost importance to ascertain, as far as may be, the limits of variation which shall still leave an assemblage under one common designation. The results of such an inquiry are admirably illustrated by the labours of Mr. Davidson in the Fossil Brachiopoda. That gentleman has contributed to the Palæontographical Society of London a series of memoirs which are models of research. An accomplished draughtsman, his correct eye for form has given precision to his comparisons, while the patient labour with which he has gathered and studied large numbers of each species has had the double advantage of diminishing the chaos of palæontological nomenclature and of illustrating the mode in which all inquiries in natural history ought to be conducted.

One striking result of his work is, that satisfactory reason appears for distrusting the mere difference of locality as an index of specific difference. At first sight the inference that the forms occurring in two distinct localities must be different, seems natural enough, so long as the exact contemporaneity of the two forms is assumed. But when it is remembered that the two deposits—say the carboniferous or the permian rocks of England on the one hand, and Germany on the other—may not have been laid down at the same time, but in

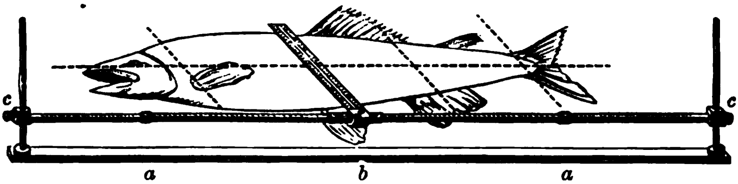
succession the one to the other, it is apparent that the inhabitants of the English may have migrated to the German area, or the reverse. But migration implies time; and it may be said that during that time such variation may have taken place that the *émigré* is in fact a new species. But then that decision has no basis so long as we are ignorant of the time which has elapsed during the migration, and of the time necessary for the development of a new species. I assume, it will be observed, that such development takes place according to the hypothesis, by modification of some type already existing, as it will appear from what follows that for the closely adjacent areas with which I am about to deal, it is immaterial whether this, or the hypothesis of specific creation be adopted.

But even assuming the exact contemporaneity of the deposits containing two nearly allied forms, on what data is the assertion grounded that these two are distinct? The only satisfactory data would be copious and precise information as to the limits of variation of each form within its own area, and the demonstration therefrom that no such intermediate steps existed as would prove the passage from one to the other, both zoologically and geographically. Where the number of individuals in each area is large, their comparison may be decisive. But where, as is more frequent, the number is small, some standard must be found—some scale adjusted, by reference to which the value of the differences may be determined. How is such a scale to be procured? Only, I think, by the slow, not brilliant, but eminently useful process of local observation—by the careful, repeated, and exact, measurement of all available specimens of well-defined species found in one district, and by the construction, from these measurements, of tables of variations. To the necessity for such tables I was led while studying the fossil fishes of palæozoic and earlier secondary age. Among the published descriptions of species I found absolute size taken as a leading character in cases where such a test was as applicable as it would be in separating specifically a kitten and a cat, and far less so than in distinguishing species among men of a large town. With large size are not unfrequently given, as of equal value, greater robustness of bones and thickness of scales—characters which, in fact, were the necessary concomitants of larger size.

Again, it was stated that the distances of the fins from the head, and their relative positions to each other, are important distinctions; but a very cursory inspection of a number of trouts, powans or goldfish, make this by no means a fixed quantity. The same may be said of the depth of the body and the thickness of the tail-root.

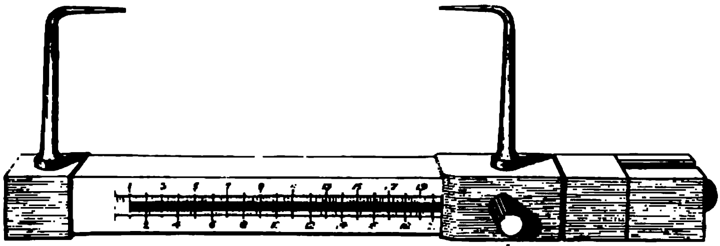
Impressed with the importance of collecting facts from which my

belief in the specific identity of certain English and German palæozoic fish might be confirmed or disproved, I thought of some instrument by which measurements on a large scale might be made with absolute precision, or by which at least the chances of error in every case might be equalized. Professor Huxley, I found, had designed an instrument consisting of a frame such as is used for writing-slates, with pointers fixed to its inner margins. But this form was difficult of application to any great range of sizes. The instrument which I now exhibit was devised with the help of Mr. James Jordan, of the Mining Record Office, who also constructed it with great care. The idea is very simple. A fixed metal base supports at either end a vertical rod, on which slides up and down a metal box (*c*, *c*), fixed at any height by a small screw, and bearing between them a brass rod carefully graduated. When the base (which in the instrument exhibited is too slight) is placed on the table, the fish to be measured is laid on its side parallel to the graduated rod; on the rod slides back and forward a brass ring (*b*), from which projects at right angles a graduated ivory scale. To fix the fish in perfect parallelism to the rod, it is only necessary to measure the distance of the muzzle and of the middle of the tail from the rod, and to make both distances the same. By shifting the ivory scale back and forwards, it is possible at one operation to record the measurement taken, and the distance from the head at which it is taken, while the sliding rings (*a*, *a*) register the points to and from which the measurement is made. Thus it is easy to construct a diagram of the outline. Thus, calling the figure of a fish approximately an ellipse, the brass rod



is the transverse axis of the figure, the minor axes and parallels to them are at once fixed and measured, and the relative distances of particular points read off, without disturbing the instrument. It may be said that the depth of body is exaggerated, or at least altered, by the animal being laid on its side; but this does not affect proportional measurements, since all are similarly affected; moreover, it is impossible to get this dimension except in the living fish in its own element, and that is not an easy task; any attempt to do so with a dead fish, by suspending or fixing it in any way, is at once cumbrous and liable to errors, not the least of which will follow from changing the positions of the instruments employed.

I have made frequent use of the instrument in the case of fossil fish, and found it of easy application. But London is not the best place for obtaining large numbers of fresh specimens. In Glasgow, however, it will be possible, and I shall be glad of any assistance I can procure towards the compilation of such materials as I have dwelt on the necessity for procuring. The instrument will be at their disposal, either to use, or, if any one chooses, to copy. The cost is trifling—25s., I think, covers it, and I have no doubt that Mr. Jordan could direct the preparation of others, having surmounted the initial difficulties of design.



a *b* *c*

To Mr. Jordan I owe the very ingenious instrument now exhibited for taking accurate measurements of shells. Though merely a modification of the calippers, which was the rude device I suggested to him, the dexterity with which risks are avoided is remarkable. At one end of a rectangular box is fixed an upright limb, *a*, whose extremity is bent at right angles. A corresponding limb, *b*, slides on the box, and is steadied by a pin passing through a double slot. The instrument is graduated along either side in 1-50th of an inch, and the horizontal tips of the limbs are so adjusted that that of the fixed one corresponds to the commencement of the scale, while that of the moveable limb corresponds to the edge of the sliding case; so that when an object is placed between the lips, its true dimension is read off on [illegible] vered part of the scale. The moveable limb is adjust [illegible] ns, the instrument being held so that the thumb is [illegible] end to which the fixed limb is attached, whil [illegible] side controls the free limb. But as many [illegible] very slight pressure would crush them, M [illegible] the l[illegible] play on a short thick elastic cord, [illegible] relieved as soon as the fingers [illegible] length of the scale available f[illegible] piece, *c*, has been provided, by [illegible] may be removed the requi[illegible]

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ring seemed no further from perfection than
a solid india-rubber ring of the same shape,

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In conclusion, let me venture again to urge the importance of local collections. Our museums are incomplete so long as they do not contain specimens of the same species, collected in many localities, and thus illustrative not merely of variation, but also of the conditions under which these variations occur. The instructive lithological series collected by my friend, Mr. Young, and exhibited in the Hunterian Museum, offer an example which I would gladly see extended to every department of the Natural History of Western Scotland.

VII.—*On Vortex Atoms.* By PROFESSOR SIR WILLIAM THOMSON.

Read March 6, 1867.

AFTER noticing Helmholtz's admirable discovery of the law of vortex motion in a perfect liquid—that is, in a fluid perfectly destitute of viscosity (or fluid friction)—the author said that this discovery inevitably suggests the idea that Helmholtz's rings are the only true atoms. For, the only pretext seeming to justify the monstrous assumption of infinitely strong and infinitely rigid pieces of matter, the existence of which is asserted as a probable hypothesis by some of the greatest modern chemists in their rashly-worded introductory statements, is that urged by Lucretius, and adopted by Newton, that it seems necessary to account for the unalterable distinguishing qualities of different kinds of matter. But Helmholtz has proved an absolutely unalterable quality in the motion of any portion of a perfect liquid, in which the peculiar motion which he calls "wirbel-bewegung" has been once created. Thus, any portion of a perfect liquid which has "wirbel-bewegung" has one recommendation of Lucretius' atoms—infinitely perennial specific quality. To generate or to destroy "wirbel-bewegung" in a perfect fluid can only be an act of creative power. Lucretius' atom does not explain any of the properties of matter without attributing them to the atom itself. Thus the "clash of atoms," as it has been well called, has been invoked by his modern followers to account for the elasticity of gases. Every other property of matter has similarly required an assumption of specific forces pertaining to the atom. It is as easy (and as improbable—not more so) to assume whatever specific forces may be required in any portion of matter which possesses the "wirbel-bewegung," as in a solid indivisible piece of matter; and hence the Lucretius atom has no *prima facie* advantage over the Helmholtz atom. A magnificent display of smoke-rings, which he recently had the pleasure of witnessing in Professor Tait's lecture-room, diminished by one the number of assumptions required to explain the properties of matter, on the hypothesis that all bodies are composed of vortex atoms in a perfect homogeneous liquid. Two smoke-rings were frequently seen to bound obliquely from one another, shaking violently from the effects of the shock. The result was very similar to that observable in two large india-rubber rings striking one another in the air. The elasticity of each smoke-ring seemed no further from perfection than might be expected in a solid india-rubber ring of the same shape,

from what we know of the viscosity of india-rubber. Of course, this kinetic elasticity of form is perfect elasticity for vortex rings in a perfect liquid. It is at least as good a beginning as the "clash of atoms" to account for the elasticity of gases. Probably the beautiful investigations of D. Bernouilli, Herapath, Joule, Krönig, Clausius, and Maxwell, on the various thermo-dynamic properties of gases, may have all the positive assumptions they have been obliged to make as to mutual forces between two atoms and kinetic energy acquired by individual atoms or molecules, satisfied by vortex rings, without requiring any other property in the matter whose motion composes them than inertia and incompressible occupation of space. A full mathematical investigation of the mutual action between two vortex rings of any given magnitudes and velocities, passing one another in any two lines, so directed that they never come nearer one another than a large multiple of the diameter of either, is a perfectly solvable mathematical problem; and the novelty of the circumstances contemplated presents difficulties of an exciting character. Its solution will become the foundation of the proposed new kinetic theory of gases. The possibility of founding a theory of elastic solids and liquids on the dynamics of more closely packed vortex atoms may be reasonably anticipated. It may be remarked, in connection with this anticipation, that the mere title of Rankine's paper on "Molecular Vortices," communicated to the Royal Society of Edinburgh in 1849 and 1850, was a most suggestive step in physical theory.

Diagrams and wire models were shown to the Society, to illustrate knotted or knitted vortex atoms, the endless variety of which is infinitely more than sufficient to explain the varieties and allotropies of known simple bodies and their mutual affinities. It is to be remarked that two ring atoms linked together, or one knotted in any manner with its ends meeting, constitute a system which, however it may be altered in shape, can never deviate from its own peculiarity of multiple continuity, it being impossible for the matter in any line of vortex motion to go through the line of any other matter in such motion, or any other part of its own line. In fact, a closed line of vortex core is literally indivisible by any action resulting from vortex motion.

The author called attention to a very important property of the vortex atom, with reference to the now celebrated spectrum analysis practically established by the discoveries and labours of Kirchof and Bunsen. The dynamical theory of this subject, which Professor Stokes had taught to the author of the present paper before September, 1852, and which he has taught in his lectures in the University of Glasgow from that time forward, required that the ultimate constitution of

simple bodies should have one or more fundamental periods of vibration, as has a stringed instrument of one or more strings, or an elastic solid, consisting of one or more tuning forks rigidly connected. To assume such a property in the Lucretius atom, is at once to give it that very flexibility and elasticity, for the explanation of which, as exhibited in aggregate bodies, the atomic constitution was originally assumed. If, then, the hypothesis of atoms and vacuum imagined by Lucretius and his followers to be necessary to account for the flexibility and compressibility of tangible solids and fluids, were really necessary, it would be necessary that the molecule of sodium, for instance, should be not an atom, but a group of atoms, with void space between them. Such a molecule could not be strong and durable; and thus it loses the one recommendation which has given it the degree of acceptance it has had among philosophers; but, as the experiments shown to the Society illustrate, the vortex atom has perfectly definite fundamental modes of vibration, depending solely on that motion, the existence of which constitutes it. The discovery of these fundamental modes forms an intensely interesting problem of pure mathematics. Even for a simple Helmholtz ring, the analytical difficulties which it presents are of a very formidable character, but certainly far from insuperable in the present state of mathematical science. The author of the present communication had not attempted, hitherto, to work it out except for an infinitely long, straight, cylindrical vortex. For this case he is working out solutions corresponding to every possible description of infinitesimal vibration, and intended to include them in a mathematical paper which he hoped soon to be able to communicate to the Royal Society. One very simple result which he could now state is the following:—Let such a vortex be given, with its section differing from exact circular figure by an infinitesimal harmonic deviation of order, i. This *form* will travel as waves round the axis of the cylinder in the same direction as the vortex rotation, with an angular velocity equal to $\frac{i-1}{i}$ of the angular velocity of this rotation. Hence, as the number of crests in a whole circumference is equal to i, for a harmonic deviation of order i, there are $i-1$ periods of vibration in the period of revolution of the vortex. For the case $i=1$ there is no vibration, and the solution expresses merely an infinitesimally displaced vortex with its circular form unchanged. The case $i=2$ corresponds to elliptic deformation of the circular section; and for it the period of vibration is simply therefore the period of revolution. These results are, of course, applicable to the Helmholtz ring when the diameter of the approximately circular section is small in comparison with the

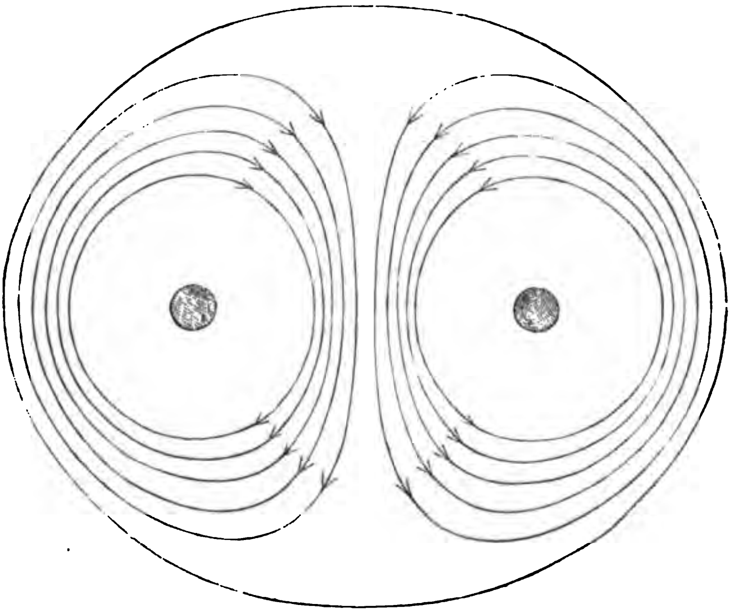
diameter of the ring, as it is in the smoke-rings exhibited to the Society. The lowest fundamental modes of the two kinds of transverse vibrations of a ring, such as the vibrations that were seen in the experiments, must be much graver than the elliptic vibration of section. It is probable that the vibrations which constitute the incandescence of sodium vapour are analogous to those which the smoke-rings had exhibited; and it is therefore probable that the period of the vortex rotations of the atoms of sodium vapour are much less than $\frac{1}{525}$ of the millionth of the millionth of a second, this being approximately the period of vibration of the yellow sodium light. Further, inasmuch as this light consists of two sets of vibrations co-existent in slightly different periods, equal approximately to the time just stated, and of as nearly as can be perceived equal intensities; the sodium atom must have two fundamental modes of vibration, having those for their respective periods, and being about equally excitable by such forces as the atom experiences in the incandescent vapour. This last condition renders it probable that the two fundamental modes concerned are approximately similar (and not merely different orders of different series chancing to concur very nearly in their periods of vibration). In an approximately circular and uniform disc of elastic solid the fundamental modes of transverse vibration, with nodal division into quadrants, fulfils both the conditions. In an approximately circular and uniform ring of elastic solid these conditions are fulfilled for the flexural vibrations in its plane, and also in its transverse vibrations perpendicular to its own plane. But the circular vortex ring, if created with one part somewhat thicker than another, would not remain so, but would experience longitudinal vibrations round its own circumference, and could not possibly have two fundamental modes of vibration similar in character and approximately equal in period. The same assertion may, it is probable,* be practically extended to any atom consisting of a single vortex ring, however involved, as illustrated by those of the models shown to the Society, which consisted of only a single wire, knotted in various ways. It seems, therefore, probable that the sodium atom may not consist of a single vortex line, but it may very probably consist of two approximately equal vortex rings passing through one another, like two links of a chain. It is, however, quite certain that a vapour consisting of such atoms, with proper volumes and angular velocities in the two rings of each atom, would act precisely as incan-

* *April* 26, 1867.—The author has recently seen reason for believing that the sodium characteristic might be realized by a certain configuration of a single line of vortex core, to be described in the mathematical paper which he intends to communicate to the Royal Society of Edinburgh.

descent sodium vapour acts: that is to say, would fulfil the "spectrum test" for sodium.

The possible effect of change of temperature on the fundamental modes cannot be pronounced upon without mathematical investigation not hitherto executed; and therefore we cannot say that the dynamical explanation now suggested is mathematically demonstrated so far as to include the very approximate identity of the periods of the vibrating particles of the incandescent vapour with those of their corresponding fundamental modes, at the lower temperature, at which the vapour exhibits its remarkable absorbing power for the sodium light.

A very remarkable discovery made by Helmholtz in the simple vortex ring is, that it always moves, relatively to the distant parts of the fluid, in a direction perpendicular to its plane towards the side towards which the rotatory motion carries the inner parts of the ring. The determination of the velocity of this motion, even approximately for rings of which the sectional radius is small in comparison with the radius of the circular axis, has presented mathematical difficulties which have not yet been overcome. In the smoke rings which have been



actually observed, it seems to be always something smaller than the velocity of the fluid along the straight axis through the centre of the ring; for the observer, standing beside the line of motion of

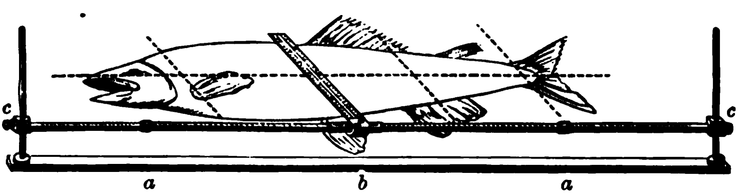
succession the one to the other, it is apparent that the inhabitants of the English may have migrated to the German area, or the reverse. But migration implies time; and it may be said that during that time such variation may have taken place that the *émigré* is in fact a new species. But then that decision has no basis so long as we are ignorant of the time which has elapsed during the migration, and of the time necessary for the development of a new species. I assume, it will be observed, that such development takes place according to the hypothesis, by modification of some type already existing, as it will appear from what follows that for the closely adjacent areas with which I am about to deal, it is immaterial whether this, or the hypothesis of specific creation be adopted.

But even assuming the exact contemporaneity of the deposits containing two nearly allied forms, on what data is the assertion grounded that these two are distinct? The only satisfactory data would be copious and precise information as to the limits of variation of each form within its own area, and the demonstration therefrom that no such intermediate steps existed as would prove the passage from one to the other, both zoologically and geographically. Where the number of individuals in each area is large, their comparison may be decisive. But where, as is more frequent, the number is small, some standard must be found—some scale adjusted, by reference to which the value of the differences may be determined. How is such a scale to be procured? Only, I think, by the slow, not brilliant, but eminently useful process of local observation—by the careful, repeated, and exact, measurement of all available specimens of well-defined species found in one district, and by the construction, from these measurements, of tables of variations. To the necessity for such tables I was led while studying the fossil fishes of palæozoic and earlier secondary age. Among the published descriptions of species I found absolute size taken as a leading character in cases where such a test was as applicable as it would be in separating specifically a kitten and a cat, and far less so than in distinguishing species among men of a large town. With large size are not unfrequently given, as of equal value, greater robustness of bones and thickness of scales—characters which, in fact, were the necessary concomitants of larger size.

Again, it was stated that the distances of the fins from the head, and their relative positions to each other, are important distinctions; but a very cursory inspection of a number of trouts, powans or goldfish, make this by no means a fixed quantity. The same may be said of the depth of the body and the thickness of the tail-root.

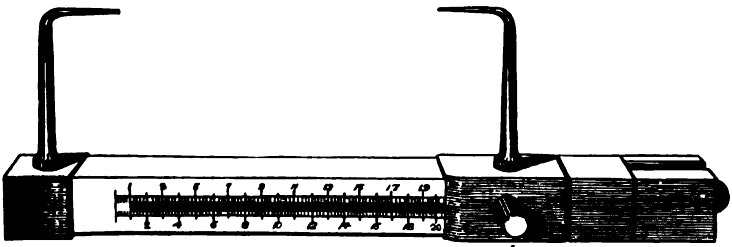
Impressed with the importance of collecting facts from which my

belief in the specific identity of certain English and German palæozoic fish might be confirmed or disproved, I thought of some instrument by which measurements on a large scale might be made with absolute precision, or by which at least the chances of error in every case might be equalized. Professor Huxley, I found, had designed an instrument consisting of a frame such as is used for writing-slates, with pointers fixed to its inner margins. But this form was difficult of application to any great range of sizes. The instrument which I now exhibit was devised with the help of Mr. James Jordan, of the Mining Record Office, who also constructed it with great care. The idea is very simple. A fixed metal base supports at either end a vertical rod, on which slides up and down a metal box (*c*, *c*), fixed at any height by a small screw, and bearing between them a brass rod carefully graduated. When the base (which in the instrument exhibited is too slight) is placed on the table, the fish to be measured is laid on its side parallel to the graduated rod; on the rod slides back and forward a brass ring (*b*), from which projects at right angles a graduated ivory scale. To fix the fish in perfect parallelism to the rod, it is only necessary to measure the distance of the muzzle and of the middle of the tail from the rod, and to make both distances the same. By shifting the ivory scale back and forwards, it is possible at one operation to record the measurement taken, and the distance from the head at which it is taken, while the sliding rings (*a*, *a*) register the points to and from which the measurement is made. Thus it is easy to construct a diagram of the outline. Thus, calling the figure of a fish approximately an ellipse, the brass rod



is the transverse axis of the figure, the minor axes and parallels to them are at once fixed and measured, and the relative distances of particular points read off, without disturbing the instrument. It may be said that the depth of body is exaggerated, or at least altered, by the animal being laid on its side; but this does not affect proportional measurements, since all are similarly affected; moreover, it is impossible to get this dimension except in the living fish in its own element, and that is not an easy task; any attempt to do so with a dead fish, by suspending or fixing it in any way, is at once cumbrous and liable to errors, not the least of which will follow from changing the positions of the instruments employed.

I have made frequent use of the instrument in the case of fossil fish, and found it of easy application. But London is not the best place for obtaining large numbers of fresh specimens. In Glasgow, however, it will be possible, and I shall be glad of any assistance I can procure towards the compilation of such materials as I have dwelt on the necessity for procuring. The instrument will be at their disposal, either to use, or, if any one chooses, to copy. The cost is trifling—25s., I think, covers it, and I have no doubt that Mr. Jordan could direct the preparation of others, having surmounted the initial difficulties of design.



To Mr. Jordan I owe the very ingenious instrument now exhibited for taking accurate measurements of shells. Though merely a modification of the calippers, which was the rude device I suggested to him, the dexterity with which risks are avoided is remarkable. At one end of a rectangular box is fixed an upright limb, *a*, whose extremity is bent at right angles. A corresponding limb, *b*, slides on the box, and is steadied by a pin passing through a double slot. The instrument is graduated along either side in 1-50th of an inch, and the horizontal tips of the limbs are so adjusted that that of the fixed one corresponds to the commencement of the scale, while that of the moveable limb corresponds to the edge of the sliding case; so that when an object is placed between the lips, its true dimension is read off on the uncovered part of the scale. The moveable limb is adjusted by two lateral pins, the instrument being held so that the thumb is applied against the end to which the fixed limb is attached, while a finger on either side controls the free limb. But as many shells are so delicate that very slight pressure would crush them, Mr. Jordan has caused the limb to play on a short thick elastic cord, so that the pressure on the shell is relieved as soon as the fingers are withdrawn. To render the whole length of the scale available in measuring large specimens, an extra piece, *c*, has been provided, by which the extremity of the elastic band may be removed the requisite distance outwards.

In drawing attention to the elegance and ingenuity of this little

instrument, I must at the same time point out that its uses are more limited than those of the previous instrument, and that there are certain kinds of dimension of which it takes no cognizance. It is a delicate and easily applied means of determining the absolute dimensions of shells, but can give no account of their proportions; hence it cannot supply the materials for constructing the figure of the shell. But so far as it goes, the information it does supply is of value, especially where it is employed in comparing the same species, and those very prolific ones, from several localities.

In using the instrument the shell is held vertically, the fixed point applied to the centre of the free edge of the lip, and the other point adjusted to the middle of the body whorl, the extreme diameter being arrived at, as with callipers, by shifting to and fro. The shell is next held horizontally, and the fixed tip placed on the umbilicus, the opposite point resting on the apex.

Helix aspersa.—Of this species 167 specimens, in the collection of Mr. Bone, of Camden Town, were examined. The localities were 23 in number.

Hammersmith, range of height to breadth at lowest whorl, 1 : 1·06 —1 : 1·8; in other words, the shell was either as high as broad, or its breadth was nearly twice its height.

Richmond averaged,		. . . . . 1 : 1·8
At Kingston on Thames,		. . . . 1 : 1·7—1 : 1·9
At Hampstead, average		. . . . 1 : 1·7
At Ramsgate,	,,	. . . . 1 : 1·5—1 : 1·9
Isle of Wight,	,,	. . . . 1 : 1·5—1 : 1·9
Beaumaris,	,,	. . . . 1 : 2 —1 : 1·5

The largest specimens were obtained from Hammersmith, Bow to the east of London, and Sandown (Isle of Wight). Dimensions: $\frac{96}{123}$ $\frac{91}{142}$ $\frac{94}{134}$ $\frac{\text{Height}}{\text{Breadth at base.}}$ The smallest were from Arundel, averaging $\frac{55}{102}$. Those from the counties along the Severn were, as a whole, inferior in height to those of the other regions. The average of the species for England may be taken as $\frac{70-75}{120-130}$. It is worthy of note, that in young specimens the ratio of height to breadth was more nearly one of equality over all—never falling so low as 1 : 1·7, and reaching as high as 1 : 2·05.

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The author called attention to a very important property of the vortex atom, with reference to the now celebrated spectrum analysis practically established by the discoveries and labours of Kirchof and Bunsen. The dynamical theory of this subject, which Professor Stokes had taught to the author of the present paper before September, 1852, and which he has taught in his lectures in the University of Glasgow from that time forward, required that the ultimate constitution of

simple bodies should have one or more fundamental periods of vibration, as has a stringed instrument of one or more strings, or an elastic solid, consisting of one or more tuning forks rigidly connected. To assume such a property in the Lucretius atom, is at once to give it that very flexibility and elasticity, for the explanation of which, as exhibited in aggregate bodies, the atomic constitution was originally assumed. If, then, the hypothesis of atoms and vacuum imagined by Lucretius and his followers to be necessary to account for the flexibility and compressibility of tangible solids and fluids, were really necessary, it would be necessary that the molecule of sodium, for instance, should be not an atom, but a group of atoms, with void space between them. Such a molecule could not be strong and durable; and thus it loses the one recommendation which has given it the degree of acceptance it has had among philosophers; but, as the experiments shown to the Society illustrate, the vortex atom has perfectly definite fundamental modes of vibration, depending solely on that motion, the existence of which constitutes it. The discovery of these fundamental modes forms an intensely interesting problem of pure mathematics. Even for a simple Helmholtz ring, the analytical difficulties which it presents are of a very formidable character, but certainly far from insuperable in the present state of mathematical science. The author of the present communication had not attempted, hitherto, to work it out except for an infinitely long, straight, cylindrical vortex. For this case he is working out solutions corresponding to every possible description of infinitesimal vibration, and intended to include them in a mathematical paper which he hoped soon to be able to communicate to the Royal Society. One very simple result which he could now state is the following:—Let such a vortex be given, with its section differing from exact circular figure by an infinitesimal harmonic deviation of order, i. This *form* will travel as waves round the axis of the cylinder in the same direction as the vortex rotation, with an angular velocity equal to $\frac{i-1}{i}$ of the angular velocity of this rotation. Hence, as the number of crests in a whole circumference is equal to i, for a harmonic deviation of order i, there are $i-1$ periods of vibration in the period of revolution of the vortex. For the case $i=1$ there is no vibration, and the solution expresses merely an infinitesimally displaced vortex with its circular form unchanged. The case $i=2$ corresponds to elliptic deformation of the circular section; and for it the period of vibration is simply therefore the period of revolution. These results are, of course, applicable to the Helmholtz ring when the diameter of the approximately circular section is small in comparison with the

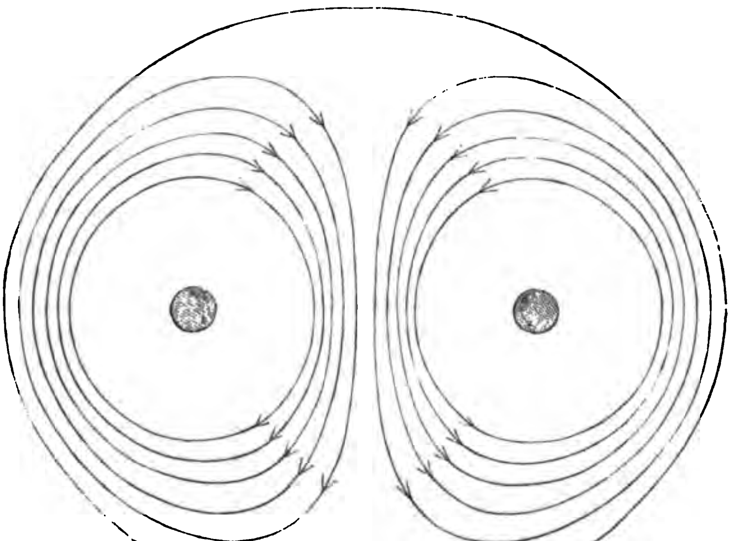
diameter of the ring, as it is in the smoke-rings exhibited to the Society. The lowest fundamental modes of the two kinds of transverse vibrations of a ring, such as the vibrations that were seen in the experiments, must be much graver than the elliptic vibration of section. It is probable that the vibrations which constitute the incandescence of sodium vapour are analogous to those which the smoke-rings had exhibited; and it is therefore probable that the period of the vortex rotations of the atoms of sodium vapour are much less than $\frac{1}{525}$ of the millionth of the millionth of a second, this being approximately the period of vibration of the yellow sodium light. Further, inasmuch as this light consists of two sets of vibrations co-existent in slightly different periods, equal approximately to the time just stated, and of as nearly as can be perceived equal intensities; the sodium atom must have two fundamental modes of vibration, having those for their respective periods, and being about equally excitable by such forces as the atom experiences in the incandescent vapour. This last condition renders it probable that the two fundamental modes concerned are approximately similar (and not merely different orders of different series chancing to concur very nearly in their periods of vibration). In an approximately circular and uniform disc of elastic solid the fundamental modes of transverse vibration, with nodal division into quadrants, fulfils both the conditions. In an approximately circular and uniform ring of elastic solid these conditions are fulfilled for the flexural vibrations in its plane, and also in its transverse vibrations perpendicular to its own plane. But the circular vortex ring, if created with one part somewhat thicker than another, would not remain so, but would experience longitudinal vibrations round its own circumference, and could not possibly have two fundamental modes of vibration similar in character and approximately equal in period. The same assertion may, it is probable,* be practically extended to any atom consisting of a single vortex ring, however involved, as illustrated by those of the models shown to the Society, which consisted of only a single wire, knotted in various ways. It seems, therefore, probable that the sodium atom may not consist of a single vortex line, but it may very probably consist of two approximately equal vortex rings passing through one another, like two links of a chain. It is, however, quite certain that a vapour consisting of such atoms, with proper volumes and angular velocities in the two rings of each atom, would act precisely as incan-

* *April* 26, 1867.—The author has recently seen reason for believing that the sodium characteristic might be realized by a certain configuration of a single line of vortex core, to be described in the mathematical paper which he intends to communicate to the Royal Society of Edinburgh.

descent sodium vapour acts: that is to say, would fulfil the "spectrum test" for sodium.

The possible effect of change of temperature on the fundamental modes cannot be pronounced upon without mathematical investigation not hitherto executed; and therefore we cannot say that the dynamical explanation now suggested is mathematically demonstrated so far as to include the very approximate identity of the periods of the vibrating particles of the incandescent vapour with those of their corresponding fundamental modes, at the lower temperature, at which the vapour exhibits its remarkable absorbing power for the sodium light.

A very remarkable discovery made by Helmholtz in the simple vortex ring is, that it always moves, relatively to the distant parts of the fluid, in a direction perpendicular to its plane towards the side towards which the rotatory motion carries the inner parts of the ring. The determination of the velocity of this motion, even approximately for rings of which the sectional radius is small in comparison with the radius of the circular axis, has presented mathematical difficulties which have not yet been overcome. In the smoke rings which have been



actually observed, it seems to be always something smaller than the velocity of the fluid along the straight axis through the centre of the ring; for the observer, standing beside the line of motion of

the ring, sees, as its plane passes through the position of his eye, a convex outline of an atmosphere of smoke in front of the ring. This convex * outline indicates the bounding surface between the quantity of smoke which is carried forward with the ring in its motion and the surrounding air which yields to let it pass. It is not so easy to distinguish the corresponding convex outline behind the ring, because a confused trail of smoke is generally left in the rear. In a perfect fluid the bounding surface of the portion carried forward would necessarily be quite symmetrical on the anterior and posterior sides of the middle plane of the ring. The motion of the surrounding fluid must be precisely the same as it would be if the space within this surface were occupied by a smooth solid; but in reality the air within it is in a state of rapid motion, circulating round the circular axis of the ring with increasing velocity on the circuits nearer and nearer to the ring itself. The circumstances of the actual motion may be imagined thus:—Let a solid column of india-rubber, of circular section, with a diameter small in proportion to its length, be bent into a circle, and its two ends properly spliced together, so that it may keep the circular shape when left to itself; let the aperture of the ring be closed by an infinitely thin film; let an impulsive pressure be applied all over this film, of intensity so distributed as to produce the definite motion of the fluid specified as follows, and instantly thereafter let the film be all liquefied. This motion is, in accordance with one of Helmholtz's laws, to be along those curves which would be the lines of force, if, in place of the india-rubber circle, were substituted a ring electro-magnet,† and the velocities at different points are to be in proportion to the intensities of the magnetic forces in the corresponding points of the magnetic field. The motion, as has been long known, will fulfil this definition, and will continue fulfilling it, if the initiating velocities at every point of the film perpendicular to its own plane be in proportion to the inten-

* The diagram represents precisely the convex outline referred to, and the lines of motion of the interior fluid carried along by the vortex, for the case of a double vortex consisting of two infinitely long parallel straight vortices of equal rotations in opposite directions. The curves have been drawn by Mr. D. M'Farlane, from calculations which he has performed by means of the equation of the system of curves, which is—

$$\frac{y^2}{a^2} = \frac{2x}{a} \cdot \frac{N+1}{N-1} - \left(1 + \frac{x^2}{a^2}\right), \text{ where } \log. N = \frac{x+b}{a}.$$

The proof will be given in the mathematical paper which the author intends to communicate in a short time to the Royal Society of Edinburgh.

† That is to say, a circular conductor with a current of electricity maintained circulating through it.

sities of the magnetic force in the corresponding points of the magnetic field. Let, now, the ring be moved perpendicular to its own plane in the direction *with* the motion of the fluid, through the middle of the ring, with a velocity very small in comparison with that of the fluid at the centre of the ring. A large approximately globular portion of the fluid will be carried forward with the ring. Let the velocity of the ring be increased; the volume of fluid carried forward will be diminished in every diameter, but most in the axial or fore-and-aft diameter, and its shape will thus become sensibly oblate. By increasing the velocity of the ring forward more and more this oblateness will increase, until, instead of being wholly convex, it will be concave, before and behind, round the two ends of the axis. If the forward velocity of the ring be increased until it is just equal to the velocity of the fluid, through the centre of the ring, the axial section of the outline of the portion of fluid carried forward will become a lemniscate. If the ring be carried still faster forward, the portion of it carried with the india-rubber ring will be itself annular; and, relatively to the ring, the motion of the fluid will be backwards through the centre. In all cases the figure of the portion of fluid carried forward, and the lines of motion, will be symmetrical, both relatively to the axis and relatively to the two sides of the equatorial plane. Any one of the states of motion thus described might of course be produced either in the order described, or by first giving a velocity to the ring, and then setting the fluid in motion by aid of an instantaneous film, or by applying the two initiative actions simultaneously. The whole amount of the impulse required, or, as we may call it, the effective momentum of the motion, or simply the momentum of the motion, is the sum of the integral values of the impulses on the ring and on the film required to produce one or other of the two components of the whole motion. Now, it is obvious that, as the diameter of the ring is very small in comparison with the diameter of the circular axis, the impulse on the ring must be very small in comparison with the impulse on the film, unless the velocity given to the ring is much greater than that given to the central parts of the film. Hence, unless the velocity given to the ring is so very great as to reduce the volume of the fluid carried forward with it to something not incomparably greater than the volume of the solid ring itself, the momenta of the several configurations of motions we have been considering will exceed by but insensible quantities the momentum when the ring is fixed. The value of this momentum is easily found by a proper application of Green's formulæ. Thus the actual momentum of the portion of fluid carried forward (being the same as that of a solid of the same density moving with

the same velocity), together with an equivalent for the inertia of the fluid yielding to let it pass, is approximately the same in all these cases, and is equal to a Green's integral, expressing the whole initial impulse on the film. The equality of the effective momentum for different velocities of the ring is easily verified without analysis for velocities not so great as to cause sensible deviations from spherical figure in the portion of fluid carried forward. Thus, in every case, the length of the axis of the portion of the fluid carried forward is determined by finding the point in the axis of the ring at which the velocity is equal to the velocity of the ring. At great distances from the plane of the ring this velocity varies, as does the magnetic force of an infinitesimal magnet on a point in its axis, inversely as the cube of the distance from the centre. Hence the cube of the radius of the approximately globular portion carried forward is in simple proportion to the velocity of the ring, and therefore its momentum is constant for different velocities of the ring. To this must be added, as was proved by Poisson, a quantity equal to half its own amount, as an equivalent for the inertia of the external fluid; and the sum is the whole effective momentum of the motion. Hence we see not only that the whole effective momentum is independent of the velocity of the ring, but that its amount is the same as the magnetic moment in the corresponding ring electro-magnet. The same result is, of course, obtained by the Green's integral referred to above.

The synthetical method just explained is not confined to the case of a single circular ring specially referred to, but is equally applicable to a number of rings of any form, detached from one another, or linked through one another, in any way, or to a single line knotted to any degree and quality of "multiple continuity," and joined continuously, so as to have no end. In every possible such case the motion of the fluid at every point, whether of the vortex core or of the fluid filling all space round it, is perfectly determined by Helmholtz's formulæ, when the shape of the core is given. And the synthetic investigation now explained proves that the effective momentum of the whole fluid motion agrees, in magnitude and direction, with the magnetic moment of the corresponding electro-magnet. Hence, still considering, for simplicity, only an infinitely thin line of core, let this line be projected on each of three planes at right angles to one another. The areas of the plane circuits thus obtained (to be reckoned, according to De Morgan's rule when automic, as they will generally be) are the components of momentum perpendicular to these three planes. The verification of this result will be a good exercise on "multiple continuity." The author is not yet sufficiently acquainted with Riemann's

remarkable researches on this branch of analytical geometry, to know whether or not all the kinds of "multiple continuity" now suggested are included in his classification and nomenclature.

That part of the synthetical investigation in which a thin solid wire ring is supposed to be moving in any direction through a fluid with the free vortex motion previously excited in it, requires the diameter of the wire at every point to be infinitely small in comparison with the radius of curvature of its axis, and with the distance of the nearest of any other part of the circuit from that point of the wire. But when the effective moment of the whole fluid motion has been found for a vortex with infinitely thin core, we may suppose any number of such vortices, however near one another, to be excited simultaneously: and the whole effective momentum, in magnitude and direction will be the resultant of the momenta of the different component vortices each estimated separately. Hence we have the remarkable proposition that the effective momentum of any possible motion in an infinite incompressible fluid agrees in direction and magnitude with the magnetic moment of the corresponding electro-magnet in Helmholtz's theory. The author hopes to give the mathematical formulæ expressing and proving this statement in the more detailed paper, which he hopes soon to be able to lay before the Royal Society of Edinburgh.

The question early occurs to any one either observing the phenomena of smoke rings or investigating the theory—What conditions determine the size of the ring in any case? Helmholtz's investigation proves that the angular vortex velocity of the core varies directly as its length, or inversely as its sectional area. Hence the strength of the electric current in the electro-magnet, corresponding to an infinitely thin vortex core, remains constant, however much its length may be altered in the course of the transformations which it experiences by the motion of the fluid. Hence it is obvious that the larger the diameter of the ring, for the same volume and strength of vortex motions in an ordinary Helmholtz ring, the greater is the whole kinetic energy of the fluid, and the greater is the momentum; and we therefore see that the dimensions of a Helmholtz ring are determinate when the volume and strength of the vortex motion are given, and besides, either the kinetic energy on the momentum of the whole fluid motion due to it. Hence if, after any number of collisions or influences, a Helmholtz ring escapes to a great distance from others, and is then free, or nearly free, from vibrations, its diameter will have been increased or diminished, according as it has taken energy from, or given energy to, the others. A full theory of the swelling of vortex atoms by elevation of temperature is to be worked out from this principle.

Professor Tait's plan of exhibiting smoke rings is as follows:—A

large rectangular box, open at one side, has a circular hole of six or eight inches diameter cut in the opposite side;—a common rough packing-box of two feet cube, or thereabout, will answer the purpose very well. The open side of the box is closed by a stout towel, or piece of cloth, or by a sheet of india-rubber stretched across it. A blow on this flexible side causes a circular vortex ring to shoot out from the hole in the other side. The vortex rings thus generally are visible if the box is filled with smoke. One of the most convenient ways of doing this is to use two retorts, with their necks thrust into holes made for the purpose in one of the sides of the box. A small quantity of muriatic acid is put into one of these retorts, and of strong liquid ammonia into the other. By a spirit lamp applied from time to time to one or other of these retorts, a thick cloud of sal-ammoniac is readily maintained in the inside of the box. A curious and interesting experiment may be made with two boxes thus arranged, and placed either side by side, close to one another, or facing one another so as to project smoke rings meeting from opposite directions, or in various relative positions, so as to give smoke rings proceeding in paths inclined to one another at any angle, and passing one another at various distances. An interesting variation of the experiment may be made by using clear air without smoke in one of the boxes. The invisible vortex rings projected from it render their existence startlingly sensible when they come near any of the smoke rings proceeding from the other box.

In answer to questions by Dr. Angus Smith, of Manchester, and Dr. Allen Thomson, the author stated that the explanations of chemical affinity had not been attempted in any theory of atoms or molecules hitherto propounded; and the difference of chemical qualities presented by different substances had been one of the most perplexing mysteries of natural philosophy. If a theory of the constitution of matter is ever to be worked out from the suggestions now made, it is easy to conceive how chemical affinity may be explained through the mutual action of vortices depending on their characteristic and durable qualities of multiple continuity. The elasticity of solids must be explained by the mutual action of vortices more closely packed together than any distribution such as might produce the qualities of gases. If the elasticity of solids is explained at all, by this or any other molecular hypothesis, the explanation of crystalline qualities follows as a matter of course. It is, indeed, less easy to explain a solid absolutely free from crystalline quality (that is to say from difference of physical properties in different directions) than to explain a solid *with crystalline* quality.

VIII.—*Heights, and other Particulars of the November Meteors.* By A. S. HERSCHEL, B.A., F.R.A.S.

Read April 3, 1867.

THE earliest authentic record of the appearance of the November meteors dates from the 13th of October, O. S., corresponding to the 18th of October, N. S., in A. D. 902; when it is narrated that "an infinite number of stars scattered themselves, like rain, from right to left, and that year was called the year of stars." The meteoric shower appeared again in A. D. 931, 934, 1002, 1101, 1202, 1366, 1533, 1602, 1698, 1799, 1832, and 1833; and finally it returned, according to prediction, in the year 1866. A cycle of about a third part of a century, or more nearly 33¼ years, would account for all the appearances; and certain cases show (for example, the returns in 931 and 934, and those in 1832 and 1833) that the shower may be expected to return for two or three years in succession at the end of every such cycle.

The date of the "rain of stars" in A. D. 902 was the 18th of October, N. S., and the display in 1866 took place on the 14th of November, which amounts to a retardation of twenty-seven days in twenty-nine returns. The intervening dates completely bear out a progress of the phenomenon in the year of about one day at each return, so that, to cite none but the most recent recurrences, the shower which appeared on the morning of the 12th of November, 1799, was seen on the morning of the 13th of November in the years 1832 and 1833, and on the morning of the 14th of November last, in the year 1866. From an almost unbroken chain of evidence, the purely astronomical character of the November meteors is accordingly established. They form a group consisting of an infinite number of small individual bodies, assembled together in a stream crossing the earth's orbit at a point which, at the recent display, occupied the ecliptic longitude 231° 28′. The earth will occupy this position again on the morning of the 14th of November next, at 6h. 48m. A.M., Greenwich time. Allowing a further lapse of forty minutes to take place for the annual shifting of the place along the ecliptic where the meteors are encountered, they may be expected to reach their maximum display towards half-past seven o'clock in the morning, or very nearly at day-dawn in England, on the 14th of November, 1867.

The phase of the moon, then very nearly three days past the full, will detract something from the grandeur of the spectacle—even in

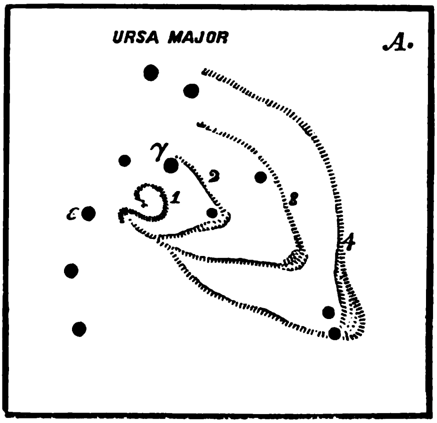
America, where the next recurrence of the shower may be expected to be visible to the best advantage; but being in the constellation Gemini, not more than two constellations distant from the radiant point or centre of divergence of the meteors, when the meteoric stream is most abundant, an excellent opportunity will present itself of directing a telescope towards the moon, which should not be neglected, in order to ascertain whether the streaks and other lucid features of the meteors preserve their lustre undimmed when projected against the bright background of the lunar disc.

Among the largest of the meteors noticed on the morning of the 14th of November, 1866, one which appeared at twenty minutes to three o'clock on that morning was seen as far south as Nottingham in England, and as far north as Aberdeen in Scotland. Of this meteor the luminous streak remained visible to the unassisted vision at least a quarter of an hour, at several widely distant places in Scotland, and in the north of England. An approximate determination of the height of the meteor, or in cases where the meteor itself escaped notice, of its streak above the surface of the earth, would evidently result from the observations at these places, if observers at the several stations had been particular to notice with sufficient care the apparent position of this extraordinary meteor by the stars; or, at least, the apparent position of its remarkably persistent train. The latter was sufficiently well observed at the Glasgow Observatory to afford, when compared with three other similar observations, which were made simultaneously at the observatories at Aberdeen and Edinburgh, and at Sunderland in Durham, a satisfactory approximation to its height. An observation of the same meteor, by Mr. T. P. Barkas at Newcastle-upon-Tyne, appears in the *Astronomical Register* for January, 1866, and was received from Mr. Barkas by Professor Grant, who informs me that it confirms, on trial, the result separately arrived at from the closely adjoining observation of the meteor made at Sunderland by Mr. T. W. Backhouse. The latter observation, as we might be led to expect from the near neighbourhood of the two places, is in every respect a perfectly parallel description to that which Mr. Barkas gives of the same meteor as it appeared at Newcastle-upon-Tyne.

The following is the appearance of the luminous streak of the large meteor as seen and described by Professor Grant in his graphic and excellent summary of the principal phenomena of the shower:—

"At 2h. 42m. the most remarkable phenomenon of this kind presented itself to us in the constellation of Ursa Major, but unfortunately none of us caught the commencement of it. The blaze of light which this meteor emitted at the time of vanishing was extraordinary. It left

behind it a curved residue of faint light, of sensible breadth, and having the form of a horse-shoe, the extremities of which at first embraced epsilon and gamma Ursæ Majoris ; but as it gradually grew fainter, it expanded in dimensions, until ultimately, before vanishing, one extremity embraced epsilon Ursæ Majoris, while the other extremity reached as far as alpha and beta of the same constellation (the two pointers). This singular phenomenon remained visible in the heavens for twenty minutes after it first attracted our attention."

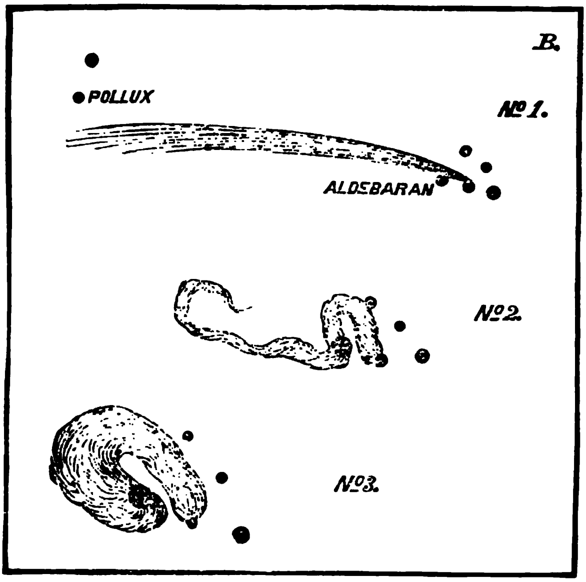


Changing appearances of a Meteoric Streak, 1866, *November* 14, as seen at the Glasgow Observatory.—1. Appearance at 2h. 42m. A.M. ; 2. Appearance at 2h. 44m. A.M. ; 3. At 2h. 48m. A.M. ; 4. Shortly before disappearance, at 2h. 52m. A.M.

In a detailed account of the meteoric shower, received from Mr. D. Gill of the Aberdeen Observatory, at King's College, by Professor Grant, the following description is given, accompanied by drawings of the meteor, and of its train :—

"2h. 40m. 58s. On my way home, when about half a mile due south of the Observatory, my attention was attracted at this instant by a glare of light. Looking up, I feared that I had missed some splendid meteor, when presently, beyond a housetop close to the east of me, appeared a most brilliant meteor, moving nearly horizontally, with an apparently slow and diminishing motion. I ran to the middle of the street, which enabled me to see backward (eastwards) along its path (indicated by its train), which appeared to have commenced somewhere between Mars and Pollux—rather nearer to the latter. The nucleus passed over alpha Tauri, rested an instant over the little pair of stars in the V, and disappeared without noise.

"I should estimate the apparent diameter of this meteor as one-fifth that of the moon; but the intensity of the light was incomparably



greater than that of any previously observed, and in its character more resembled sunlight than any other.

"The most remarkable feature, however, was the train. This was of a pale yellow colour, and at first remained as a band of dense nebulous-looking light, about half a diameter of the moon in breadth, along the path of the meteor, as in No. 1. After two minutes the train wound about as in No. 2. After three and a half minutes it had collected itself into a nebulous-looking cloud, as in No. 3, which remained vividly distinct till four minutes by the watch after the appearance of the meteor, when it was obscured by a cloud."

Drawings of the meteor and of its streak accompany Mr. D. Gill's description. In No. 1 the path of the meteor is shown, from about three degrees below Pollux to theta Tauri. In No. 2 the earlier deposited portion of the train is shown winding itself up towards Aldebaran and theta Tauri. In No. 3 the train is shown coiled up as a nebulous cloud, enfolding Aldebaran and theta Tauri.*

* Erratum.—A slight departure from the original drawing, in the engraving B, No. 3, requires to be pointed out, and corrected in this recapitulation. In the engraving B, No. 3, the streak is represented as a simple arch or curve of

A visual line from Aberdeen towards Aldebaran, about which star the nebulous cloud of light appeared to form itself at Aberdeen, and another line of sight from Glasgow towards the middle point between the stars epsilon and gamma Ursæ Majoris, where the luminous streak was seen at Glasgow, intersect each other at a height of 57 miles over a spot a few miles north of the town of Dundee, at a distance of 90 miles from the Glasgow Observatory, and 77 miles from the place of observation near the Observatory at Aberdeen.

Another excellent view of the streak was obtained by Mr. T. W. Backhouse, at Sunderland, in the county of Durham, who writes as follows:—

"On looking out of the window at 2h. 42m. A.M., I discovered the train of a meteor, the upper part being a patch of light much brighter than the rest. Fig. 2 shows it soon after I discovered it, and Fig. 3 at 2h. 44m. 40s. A.M. It was visible at 2h. 53m. I did not see the meteor, but am told that it was as bright as the moon."

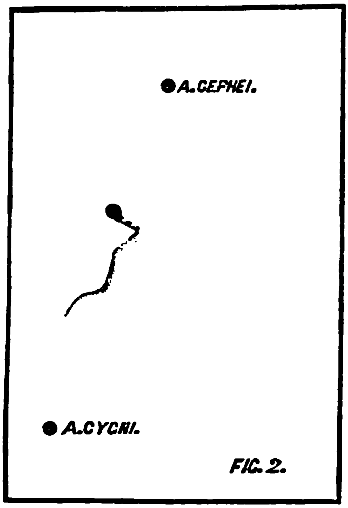


FIG. 2.

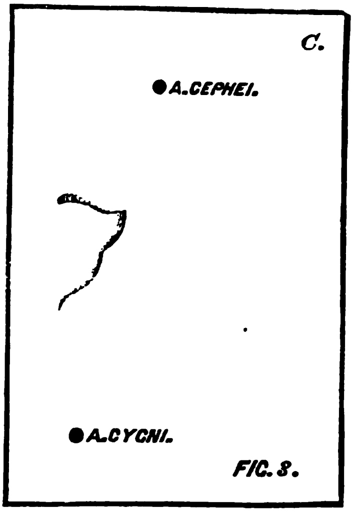


FIG. 3.

Mr. Backhouse's description of the train shows that he first saw it very near the time of its commencement, as it was visible eleven

light with a single bend. The original drawing of the streak represents it in the form of a letter ဢ, with both its branches very much compressed, and with the left branch tapering gradually to a point, and turned over the other branch like a tail. The engraving B, No. 3, is a perfectly correct representation of the phase of its appearance at Aberdeen, if the thicker or left extremity of the arch of the streak in the engraving is supposed to be not quite filled up with light.

minutes, although at such a low altitude at Sunderland as to be hardly clear from the mists of the horizon—a circumstance probably not very favourable for noting its position among the fainter stars. Two carefully-executed drawings of the streak accompany Mr. Backhouse's description. They represent the train of the meteor as it appeared at Sunderland, in shape very much resembling the figure of a comma—the bright part upwards, and the fainter part below, tapering to a point, which is curved towards the left. The position of the streak in the drawing fig. 2, when it was first seen by Mr. Backhouse, is nearly midway between the two bright stars alpha Cephei and alpha Cygni. A line of sight drawn from Sunderland towards this place intersects that drawn from the Glasgow Observatory as before, towards the middle point between epsilon and gamma Ursæ Majoris, at a height of 51 miles above the earth's surface a few miles west of Dundee. The distance of the meteoric streak from the Glasgow Observatory was in this case 80 miles, and the distance of the phenomenon from Sunderland was about 145 miles.

The distance of Aberdeen Observatory is 120 miles in a straight line, 47° north of east, from Glasgow; and the direction of Sunderland from Glasgow is 28° south of east; distant about 130 miles. From two such distant places, differing so widely in their directions from Glasgow as to lie one on the north, and the other on the south side of the meteor's line of flight, each of the two foregoing accounts possesses by itself a peculiar and independent interest. But they offer together a remarkable coincidence in the results, amounting, obviously, to an approximate determination of the meteor's height. They concur in showing that the real altitude of the meteoric streak was between 50 and 60 miles above the surface of the earth; at a distance of about 80 or 90 miles from the Glasgow Observatory; and that it was not far from the zenith of a place in the immediate neighbourhood of Dundee.

Another important account of the meteoric shower, containing an authentic description of the same meteor as observed at Edinburgh, was communicated to the Royal Astronomical Society by Professor Piazzi Smyth, in the "Monthly Notices" of the Society for December, 1866. The description is as follows:—

"Of bright meteors . . . there must have been one, about 2h. 40m. A.M., between alpha Ursæ Majoris and alpha Ursæ Minoris, for immediately thereafter the central part of its luminous track was brilliantly conspicuous, like a silver snake in the sky. From minute to minute the luminous line became more corrugated; widening and becoming fainter by degrees; and also drifting, apparently under the action of the N.W. wind blowing at the time: even after a quarter of

an hour the train matter was still visible, but changed to something like the outline of a gigantic pear, and drifted some 30° from its first position."

The description of the apparent place of the streak is only approximately given by reference to certain stars in Ursa Major and Ursa Minor, which allow a considerable latitude between them, in arc, for the place at which the phenomenon may be supposed to have been seen at Edinburgh. It is, however, again satisfactory to find that, on referring the true place of the meteoric streak to the constellations then visible above the horizon at Edinburgh, the cloud of light in its approximate position above assigned to it would appear projected against the sky at Edinburgh in a direct line between the two stars alpha Ursæ Majoris and Polaris, indicated by Professor Piazzi Smyth as including the streak between them, at a point about twice as far from the former as from the latter star. Professor Piazzi Smyth's observation of the meteor at Edinburgh, and Mr. Barkas' note of the phenomenon, as it appeared to him at Newcastle-upon-Tyne, are accordingly both corroborative of the results above arrived at on independent grounds from those accounts which are described above, from Aberdeen and Sunderland.

When the difficulty of this kind of observation is considered, especially the transient character of the impression which the momentary phenomenon of a shooting-star leaves upon the eye, it is really surprising how frequently the heights of shooting-stars have been satisfactorily determined. A list of at least two hundred well-ascertained altitudes of shooting-stars, since the first measurements of the kind were made upon ordinary shooting-stars, towards the end of the last century, might here be quoted in support of this assertion. Although presenting many anomalies, towards which errors of observation must have contributed their share, such observations show that about 60 miles above the earth's surface is the height of the region in the atmosphere, where shooting-stars are most common; and that they commonly make their appearance at a distance of about 70 miles, and vanish at a distance of rather more than 50 miles above the surface of the earth.

In this respect the large meteor of the 14th of November, whose height has just been approximately found, resembles the ordinary class of shooting-stars by being produced in the upper regions of the atmosphere. The other meteors of the same shower were doubtless quite similar to it, and to ordinary shooting-stars, in being produced by the impact of light bodies coming into collision with the outer surface of the atmosphere, which they traverse for a few moments after entering

it, with planetary speed. The nature of their light, and of the luminous streaks which they leave behind them, must, however, yet be acknowledged to remain a mystery. A spectrum-analysis of their light, which was attempted to be made, revealed nothing distinctive by which the light could be identified with any terrestrial flame. The November meteors in 1866, accordingly, differed specifically from those on the previous 10th of August, in the streaks of which the yellow Sodium-line was not only repeatedly and very distinctly seen mixed up with a diffuse spectrum, but it formed occasionally the entire light of the streak, so as to present in the spectroscope nothing but an extremely brilliant and fine yellow line of light.

No doubt appears now to exist that the November meteors form a group of light, or nearly impalpable bodies revolving round the sun in definite, but not yet determined orbits. The most probable track which they pursue is that which the Italian astronomer at Milan, Sig. G. V. Schiaparelli, has quite recently assigned to them, by the remarkable discovery that a comet, detected by Tempel on the 19th of December, 1865, which passed its perihelion in January, 1866, is in all probability a primary nucleus, of which the November meteors form the satellites. The November meteors and Tempel's comet, as calculated by Oppolzer, pass at the same epoch close to the earth's orbit, at the same ecliptic longitude, at the same obliquity to the ecliptic, and in the same direction round the sun. The periodic time of their return is also the same, so that their orbits must evidently coincide throughout their whole extent. A conspicuous and well-remembered comet, which was visible to the naked eye for two or three weeks in August and September, 1862, is likewise shown by Sig. Schiaparelli to account completely for the appearance of the meteors of the 10th of August. All the elements of their orbits agree together, almost identically, when the periodic time of the meteors is taken, as is probably the case, from ancient dates of their appearance, at about 108 years; while that of the comet, from an approximate computation at its last appearance by Oppolzer, has been ascertained to be about 123 years.

Comets and shooting-stars are accordingly regarded at present as identical bodies, or at least as connected together by some such relation as that which connects the satellites to their primaries. If this should prove on further research to be established, the long-dreaded encounter of the earth with a comet may perhaps, after all, resolve itself into nothing more than such an extraordinary shower of meteors as that which was witnessed on the morning of the 14th of November, 1866, or as that which may be expected to occur again on the morning of the 14th of November, 1867.

[redacted]

IX.—*Results obtained from Heating Scrap Iron in the Heat-Restoring Gas Furnace, and in the Furnaces commonly employed for that purpose, with Comparisons and Observations on the Quality of the Welded Iron, and on the Flame of the Common and Gas Furnaces.* By MR. WILLIAM GORMAN.

Read April 17, 1867.

IN a former paper read before this Society, the writer gave a description of the heat-restoring gas furnace; and he has now the further honour of submitting the following results and observations to the notice of the Society:—

In a day's trial of two common furnaces, 276 cwts. scrap delivered, returned 235½ cwts. welded iron, and 29½ cwts. cinder—total weight, 265 cwts.; loss in iron, 40½ cwts.; loss in weight returned, 11 cwts.; coal used in 5-feet furnace, 58⅓ cwts. per day.

In three days' trial with the gas furnace, 353 cwts. scrap delivered, returned 319 cwts. welded iron, and 56½ cwts. cinder—total weight, 375½ cwts.; loss in iron, 34 cwts.; gain in weight returned, 22½ cwts.; coal used in three days, 5 tons 8 cwt. 2 qrs., or 36 cwts. per day.

The above trials were made in cinder-bottom furnaces, in which all the cinder is collected and utilized, so that means were afforded of weighing the results and determining the loss or gain accurately.

The following particulars are calculated from the above data, and tabulated for easy reference or comparison:—

YIELD.		COMMON FURNACE.			GAS FURNACE.			GAS FURNACE. PER DAY.		
		cwts.	qrs.	lbs.	cwts.	qrs.	lbs.			
Yield, or Scrap required to produce 1 ton of Welded Iron, ...		23	1	21	22	0	14			
Iron lost in Heating, per ton, . . .		3	1	21	2	0	14			
Cinder produced, per ton, ...		2	2	0	3	2	0			
Iron lost as Vapour or Gas, ...		1	3	0	0	0	0			
								cwts.	qrs.	lbs.
One Day's work of each Furnace.	Scrap Iron delivered, .	140	2	15	140	2	15			
	Welded Iron returned,	120	0	0	126	3	26	6	3	26 more.
	Iron lost in Heating, .	20	2	15	13	2	17	6	3	26 less.
	Cinder produced, . .	15	0	0	22	0	0	7	0	0 more.
	Coal used, ...	58	1	9	36	0	0	22	1	9 less.
Iron lost as Vapour or Gas, ...		10	2	0	0	0	0	0	0	0

Yield per cent.: Common Furnace, if 235½ cwts.: 276 : : 100 will give 117·19 per cent. (cwts.)
„ „ Gas Furnace, if 319 „ : 353 : : 100 „ 110·65

Gas Furnace, less loss, ... 6·54 per cent.

It will be observed from the above table that nearly 7 cwts. more welded iron is produced in one day from the gas furnace than from the common furnace, from the same weight of scrap iron charged in each.

In the common furnace 40½ cwts. waste iron produced 29½ cwts. cinder, which cinder, if taken as a peroxide of iron, will contain only 20½ cwts. of iron, so that a ton of iron has completely disappeared, and must have burned and assumed the form of vapour before it could pass away amongst the waste gases of the furnace.

In the gas furnace 34 cwts. waste iron produced 56½ cwts. of cinder, and if the cinder be a peroxide of iron, then 34 cwts. of iron oxydized would yield 48½ cwts. cinder, which would allow 8 cwts. for fettling, &c., showing that no combustion of the iron has taken place in the gas furnace, as all the iron wasted is found in the cinder produced.

I am not aware that it has been previously observed that malleable iron disappears in the common furnace in the form of gas or vapour —the general idea is, that the iron lost in heating passes into the resulting cinder; but in the above instance only one-half of the iron wasted is found in the cinder, the other half, amounting to 10½ cwts. per day, has completely disappeared.

As referred to in a former paper on Puddling and the Bessemer Process, the combustion of iron produces an enormously high temperature, sufficient to melt malleable iron and make it run like water: consequently a layer on the surface of the iron where combustion has taken place must be overheated, or what is called practically, "burned." This layer of burned iron, which neither runs down as cinder nor flies off as vapour, is welded up with the rest, and must impair the quality of the iron.

This source of deterioration operates on all kinds of iron when welded in the common furnace, but more particularly when welding scrap, as the "burned iron" resulting from the external combustion of each piece of scrap forming a ball must be in proportion to the great surface exposed.

As the interior of a ball of scrap iron is composed of pieces an ounce or two in weight, it can easily be conceived that a large portion of the iron must be burned; and it is obvious that in heating piles or faggots of iron the same *evil* obtains, although not to so large an extent; and the oftener iron is piled and heated, the greater must be the quantity of "burned iron" produced and welded up in the body of the iron.

When iron is overheated or "burned" bodily in the course of manufacture, it can be detected; but the burned iron above referred to cannot be prevented in the common furnace, and cannot be detected or kept out of the iron when it is being welded; and as every welding heat the iron is submitted to increases the evil, in a few heatings the best and strongest iron is rendered useless for purposes where strength and tenacity are required.

Unless iron is deteriorated in some such manner as that here stated, it would seem difficult to explain an occasional occurrence in forging, when a large shaft will break across at the cold end, and drop away, while being worked under the steam-hammer. Of course, before such an event could possibly take place, the strength and tenacity of the iron must have been destroyed; and it is easier to account for it by allowing that the iron had got injured by frequent heating, and consequent debasement by the occlusion of "burnt" iron, than to suppose that the heating should be conducted in such a careless manner as to burn the iron bodily.

When iron is heated considerably above the welding point, and passed through the rolls, it will not stick together; and if hammered, will fly from under the hammer, unless struck very lightly; but by allowing the iron to cool down to the welding heat, it can be welded under the rolls or hammer, and wrought in the usual manner.

When iron is overheated in this way, it crystallizes to a certain extent, but not being fused so as to permit free mobility of its particles, the cinder or other matter contained within the iron is not expelled, but seems to fuse and interpose between the crystals, and prevent metallic contact; however that may be, the iron is crystallized and "tender," "brittle," or "frush," in some relation to the degree of heat, and perhaps also to the foreign matters contained in the iron; it is very short, both hot and cold, and no longer fit to be intrusted in good work: it will not shear without cracking, and will break with a smart stroke or fall.

"Burned iron" has all the bad qualities of fused and overheated iron in a greater degree, and without a redeeming quality. When iron is heated to such an excess in the common furnace that part of it is burned away bodily, it must be very near the fusing point, and the reason why it is not melted down may be owing to the combustion of the iron itself, as, for instance, a piece of coal while burning at a fierce white heat on the surface is quite black towards the centre, and the surfaces of the pieces of iron composing a pile may undergo combustion, and produce enormous heat, while the underlying layer may be kept at a much lower temperature by the vaporation of the iron.

Dr. Percy, in his valuable treatise on Iron and Steel, takes notice of "burned iron;" but he does not give the subject that attention its importance demands. The case adduced by the doctor is that of malleable iron melted in a crucible under slag produced in assaying iron ore. The melted iron was of a very crystalline fracture—when worked, it was very soft and tough—had a fine face, and sharp, even edges like steel. Whilst at a welding heat, also, the iron worked

very well; but on cooling to a red heat, it became cracky, and broke. This, and other experiments of a similar kind, led to the conclusion, that the property of becoming useless after exposure to a welding heat appears to be a special character of fused wrought iron.

This case does not correspond to those which occur in practice where the iron is burned in the presence of oxygen without being actually fused. When iron is completely fused, any cinder or other fusible matter which may have been worked up with the iron is set at liberty, and the iron is allowed to crystallize in its own system; besides, the iron being melted under slag, is protected from oxidation and any injurious matters contained in the furnace flame. The case is altogether different when iron is exposed and burned in an oxidizing flame (as in the common furnace), with its powerful affinity for oxygen, and that affinity intensified by enormous temperature; and although it may not be heated to the point of fusion, the above destructive agencies are sufficient to render it as weak and brittle as whinstone, and ultimately destroy its metallic properties.

The gas furnace, in producing a non-oxidizing flame in which carbonic oxide preponderates, prevents the iron from undergoing combustion, so that "burned iron" is not necessarily produced in heating; and iron may be worked to any point of excellence of which it is capable without being deteriorated by such admixture.

Having given results of practical trials of the common and gas furnaces, and observations as to the quality of iron due to each furnace, I shall now endeavour to show that the quality of *flame* of the different furnaces is adapted to produce the different results obtained—viz., to prevent combustion of the iron in the gas furnace, and to produce combustion of the iron in the common furnace.

The question has been asked, What difference can there be between the flame from coal when it is burned directly, as it is in the common furnace, and when it is first converted into gas and then burned as in the gas furnace? Are not the products of combustion the same in both, and why should there be an advantage in converting the coal into gas? The answer is, that in the common furnace, where solid fuel is burned directly down to carbonic acid, it is necessary, in order to get rapid combustion and great heat, that the air should pass rapidly through the fire, and consequently the oxygen of the air does not get saturated with the fuel; and it is found in practice that twice as much air passes through the fire as is actually required for combustion. This is a well-known fact, and has been repeatedly proven, by analyzing the products of combustion, by the most eminent chemists.

It is also well known that while that part of flame which is nearest

the fire may have a reducing action, the point or part farthest from the fire is oxidizing; and as the flame is produced over the fuel in the common furnace, and has to pass over the bridge, by the time it has reached the heating chamber the flame has begun to exert its oxidizing qualities; add to this the double amount of oxygen necessary in direct combustion, and the result is a flame having chemical constituents highly adapted to the complete destruction of iron or any other oxidizable metal; and as a proof that the top, or part of flame farthest from the fire, is the most oxidizing, I would refer to the regular experience of workmen, who find that the iron placed next the flue bridge, or part of the furnace farthest from the fire, is always wasted or cindered away more than that which is heated nearer the fire.

In the gas furnace all the coal is converted into gas before it is finally consumed, and the air for consuming the gas is heated so highly as would nearly, if not quite, melt cast iron, so that when the gas and air unite, combustion is instant and complete, and it is not necessary to supply more air than what is required for combustion.

Also, the air for burning the gas is not supplied till the gas is just entering the heating chamber, so that the first, or reducing part of the flame, is used for heating the iron; and to make sure that no combustion of the iron can possibly take place, considerably more gas is supplied than what is required for combustion or saturation of the oxygen, so that the flame of the gas furnace not only heats, but protects and nourishes the iron, carbonic oxide gas being in excess.

In heating a ton of iron in the common furnace 10 cwts. of coal is usually consumed, for which purpose $12\frac{1}{2}$ tons of air is required, $6\frac{1}{4}$ tons of which air passes through the fire unconsumed; the oxygen of the other $6\frac{1}{4}$ tons unites with the fuel to form carbonic acid gas; so that, to heat a ton of iron, 13 tons hot gases or flame passes over it, $6\frac{1}{4}$ tons being hot air, with its oxygen free.

It is found in practice that a ton of iron can be heated with half the weight of coal in the gas furnace, and the coal requires only half the air, so that 5 cwts. coal, requiring about $3\frac{1}{8}$ tons air—in all 3 tons $7\frac{1}{2}$ cwts. hot gases, with no free oxygen—heats a ton of iron in the gas furnace.

Iron at a high temperature has a powerful affinity for oxygen, and will decompose steam, or even carbonic acid gas; and when heated in such a flame as that of the common furnace, it must oxidize and burn when at a white heat, as previously shown; and many practical men, observing the resulting cinder, believe it to be impurities which the heat melts out of the iron, while it it is just so much of the iron

which is being dissolved by the oxygen passing through the furnace, and the iron left is not purer in consequence.

If iron were heated in an air-tight crucible, as when making steel, little or no cinder would be produced by the mere addition of heat; but if the same iron were exposed for so long a time as when melting in a crucible to the flame of the common furnace, no iron in a metallic form would be left in the furnace,—it would be oxidized by the flame.

As four times the weight of gaseous matter passes through the common furnace to heat a ton of iron, the current must be sixteen times stronger, so that the injurious effects of strong currents are avoided in the gas furnace, and the iron is thus placed nearly in the same conditions as if it were heated in a crucible.

No heat which could be produced in any ordinary furnace is sufficiently elevated in temperature to vaporize iron, unless oxygen were present to promote combustion of the iron itself. That the union of the oxygen and iron is promoted by forcible contact or strong currents the well-known experiment of swinging a rod of iron at a white heat rapidly through the air may be given as an illustration,—the rapid combustion of the iron which takes place giving out the beautiful scintillating light so much admired. Of course, it is the same whether the iron is driven rapidly through the air, or the air driven rapidly over the iron,—it is the forcible contact which increases combustion; and the same effect must take place in the common furnace, from the great weight and bulk of gaseous matter which passes over the iron in a given time.

Ample corroborative testimony to the above conclusions is afforded by the dissolving or washing away of the bricks composing the roof and sides of the furnace and flue,—so rapidly that some parts require to be replaced weekly. That this destruction of refractory brick is not due merely to heat and force of the gaseous current, but to the presence of oxide of iron in the flame, is evidenced by the fact that in the gas furnace, where no iron is burned or vaporized, and where the *furnace heat* is much higher, the bricks are not dissolved and as it were washed away by the gaseous current; the consequence is that the common furnace requires weekly repairs, and to be rebuilt in about nine months, while the gas furnace requires few repairs, and lasts two years.

Having thus endeavoured to show that all the conditions necessary for the complete combustion of iron are present in great force in the common furnace, and that a large amount of iron is dissipated I have already demonstrated, I would now state that the conclusions arrived at as to the deterioration of iron by the occlusion of "burned" or oxidized iron are not affected by the question, Whether a portion of

iron during combustion passes off as vapour, or oxidizes as a liquid, and afterwards escapes by sublimation?—the fact that solid or compact iron *disappears* in the common furnace by combustion evidences a much higher temperature than what can be produced by the fuel, and that consequently the surfaces of the iron where combustion takes place must be "burned."

The combustion of iron may be seen at any time in the puddling furnace. When the iron has been puddled, and is turned up from the bottom for balling, the comparatively cold iron is immediately acted on by the oxygen of the air, rises to a white heat, passes on to the point of fusion, and melts down by the fervid heat of its own combustion.

Of late it has been established by Mr. Graham, Master of the Mint, that iron at or even below a red heat can take up 4·15 times its volume of carbonic oxide gas and retain it even when cold; of course, this is a means of introducing carbon into iron without fusion or cementation, and indicates important improvements in the manufacture of iron and steel, which will not likely be neglected, and which the gas furnace provides means of following up, but for which purpose the common furnace cannot be used.

The writer regrets that he has not yet had an opportunity of following up the improvements in working iron and steel to which the above results and observations obviously lead; but he hopes that the practical iron worker will perceive, in the use of the gas furnace, a means of manufacturing iron of greatly superior quality and tensile strength to what can be produced in the common furnace; and it is expected that the above improvements will prove of more value than the established saving in coal and iron.

X.—*On Recent Discoveries in the Structure of the Internal Ear.* By Dr. Allen Thomson.

Communicated April 17, 1867.

Dr. Allen Thomson exhibited with microscopes a series of specimens illustrative of the structure of the Cochlea, made by Dr. Thomas Reid, of Glasgow, and gave an account of the recent progress of discovery in that subject, referring more particularly to the observations of Reissner, Corti, Kölliker, Henle, Claudius, Deiters, Hensen, Reid, and himself.

Dr. Thomson, after referring to the great difficulty of obtaining perfect specimens by sections of such structures as the cochlea, both on account of the minuteness and extreme delicacy of the apparatus connected with the terminal extremities of the auditory nerves, and of the combination, in the organ, of structures of very various degrees of density, described the method pursued by Dr. Reid, by means of which that observer had succeeded in obtaining sections of the cochlea in which the natural disposition of the minute parts was comparatively little disturbed. He then described the appearances exhibited by the preparations, which demonstrated in a very clear manner the following circumstances, viz.:—the form and relations of the membranous canal of the cochlea; the position and attachments of the membrane of Reissner, separating the canal from the scala vestibuli, and of the membrana basilaris lying towards the scala tympani; the structure termed membrane of Corti, together with the cells situated in the sulcus spiralis and above the organ of Corti; the points of issue of the terminal filaments of the auditory nerves, and their probable continuation into small radiated cells among the rods of the organ of Corti. The cells or rods of the organ of Corti, the cells of Deiters, the epithelial cells of the other parts of the cochlear cavities, the spiral ligament, membrana reticularis, and other structures, were also the subjects of demonstration and remarks.

In conclusion, Dr. Thomson made some remarks on the probable uses of the minute terminal nervous apparatus which had been shown, as related to the reception and combination of the vibrations of sound, in support of the view that this apparatus in the cochlea is the part of the organ of hearing more immediately connected with the distinction of the different pitch and quality of sounds.

MINUTES.

Anderson's University Buildings, November 7, 1866.

THE Sixty-fifth Session of the Philosophical Society of Glasgow was opened this evening, DR. FRANCIS H. THOMSON, the President, in the Chair.

Mr. Paul Dorn, Chemist, and Mr. Sigismund Schuman, who were proposed as members at the closing meeting of last Session, were elected.

MR. W. H. Hill and Mr. George Anderson were appointed Auditors of the Treasurer's Accounts.

MR. GEORGE ANDERSON reported, that at a recent meeting of the Committee appointed by the Society in 1863 on the subject of Free Libraries and Museums, it was considered advisable that steps should be taken, under the amended Act of 1866, for bringing the Act into operation in Glasgow; but the public feeling which had been manifested within the last few days against the increase of local taxation rendered further proceedings in present circumstances inexpedient. Mr. Anderson added, that the institution of Free Libraries was greatly facilitated by the provisions of the Act of 1866, which now only requires a majority of a public meeting, no poll being demanded, besides conferring sufficient borrowing powers for the purposes of building. On Mr. Anderson's suggestion, the Committee was continued, consisting of the President, Dr. Thomas Anderson, Dr. Allen Thomson, Dr. John Taylor, Mr. John Mayer, the Secretary, and Mr. George Anderson, Convener.

The PRESIDENT delivered an opening address "On the Workshops of Glasgow," for which, on the motion of Dr. Allen Thomson, he received the cordial thanks of the Society.

November 21, 1865.—The annual meeting of the Society for the election of Office-bearers, and for other business, was held this evening, DR. FRANCIS H. THOMSON, the President, in the Chair.

Dr. John Young, Professor of Natural History in the University of Glasgow, and Mr. A. S. Herschel, B.A., Andersonian University, were elected members.

The SECRETARY read the following Report by the Council on the state of the Society:—

REPORT BY THE COUNCIL ON THE STATE OF THE SOCIETY.

I. *Proceedings.*—The printed *Proceedings* of the Society for the Session 1865-66 amounted to 136 pages, and consisted of the following communications:—"On the most Profitable Speed for a fully Laden Cargo Steamer for a given Voyage," by Mr. James R. Napier. "On the Estimation of the Evaporative Power of Fuel," by Professor W. J. Macquorn Rankine. "On Electrically Impelled and Electrically Controlled Clocks," by Sir William Thomson. "On a Land Standard Electrometer, and a Marine and Land Telegraphic Testing Electrometer," by Sir William Thomson. "On the Determination of the Difference of Longitude between the Observations of Greenwich and Glasgow by Galvanic Signals," by Professor Grant. "On Petroleum," by the late Professor Henry D. Rogers. This was the last scientific paper from the pen of Dr. Rogers, and was sent from Boston, United States, whither the author had proceeded for the restoration of his health. When he returned home, the paper was passing through the press, and was never seen in a printed form by its author, the only portion of the *Proceedings* shown to him, a few days before his death, being a proof-sheet containing the kind allusions made to him by Dr. Allen Thomson, on the occasion of proposing the present President as his successor in the chair of the Society. The other papers embodied in the *Proceedings* were,—"On the Function of Articulate Speech, and on its connection with the Mind and the Bodily Organs," by Professor Gairdner. "On the Treatment of the Cattle Plague," by Mr. Richard Brown, which gave rise to a lively discussion, fully reported in the *Proceedings;* and "On the Luminosity of the Sea," by Messrs. David Robertson and William Keddie. Several other topics, noticed in the printed Minutes, were introduced to the Society in an incidental manner.

The following communications were not embraced in the printed *Proceedings.* Dr. Thomas Anderson gave an account of "Some Recent Changes in the Views entertained by Chemists regarding the Constitution of Chemical Compounds." Dr. Bryce described the "Geological Structure of the Earthquake District of Perthshire." Mr. W. Keddie gave an "Account of recent Observations by MM. Milne Edwards, Pasteur, and others, on the subject of so-called Spontaneous Generation." Mr. Edward A. Wünsch described "Discoveries made by himself in the Coal Formation in Arran." Dr. Allen Thomson described the "Structure of the Heart in Reptilian Animals."

II. *Lectures.*—Mr. John Zephaniah Bell, of London, was engaged by the Society to deliver two Public Lectures on the Fine Arts. Although the attendance was numerous, the experiment was only partially successful; and a deficiency in the revenue from the sale of tickets was made up by the contributions of the President and Members of the Council.

III. *Number of Members.*—At the commencement of the Session last year, the number of members of the Society was 279, of whom 14 were then in arrears of their annual subscription. In the course of the Session there was an addition of 20 new members, together with one from the suspense list, making, at the close of the Session, 300 in all, being the largest number ever attained. This number has been diminished by deaths, resignations, and non-payment of the annual subscription for the year, by 18, leaving on the list, at the beginning of the present Session, 282, of whom 14 are in arrears of payment of last year's subscriptions. The rule is, that no member is allowed to resign without having paid his arrears; the practice is to strike off such members as are in arrears for two years, when there is reason to believe that payment will not be made by continuing defaulters on the roll.

The Library.—The Librarian will make a separate report on the state of the Library, which occupied much of the attention of the Council during last session. In addition to the increased outlay in the binding of books, &c., it may here be noticed that considerable additional expense was also incurred by the necessity of sending the circulars convening the meetings of the Society by post, their delivery otherwise having occasioned constant complaints of irregularity.

Accommodation of the Society.—The attention of the Society was particularly called, in the Council's report last year, to the inconvenience of the present place of meeting, and to the propriety of obtaining better accommodation for the Society, as well as greater security for the Library. Much time was spent by Committees of the Council, and the Society, in endeavouring to obtain suitable premises, more especially in co-operation with committees of the other scientific and art societies in the city, with a view to providing a suite of rooms for their joint accommodation. All these endeavours proved fruitless; and it will be for the Council to resume its inquiries this Session, either on behalf of the Philosophical Society by itself, or in conjunction with the representatives of kindred associations.

The Exhibition Fund.—Repeated endeavours were made in the course of last Session to obtain possession of the money accruing from the Exhibition of 1846, lodged in the hands of the City Corporation,

and now amounting, with interest, to £1,012. The efforts of the Council in this matter have hitherto been unavailing. The subject, however, will not be overlooked during the present session; and, as formerly, the Council have to state that they will be glad to receive any suggestions from members of the Society which may be of use in directing their further procedure.

The report was approved of by the Society.

MR. CROSSKEY, the Librarian, gave in a report on the Library. The report stated that the number of volumes in the Library is 3,875. There have been large and valuable additions of transactions from various foreign societies, viz.:—The Imperial Geographical Society, Vienna; Royal Academy of Sciences, Berlin; University of Liege, Liege; Royal Academy of Belgium; Royal Academy of Berlin; Royal Academy of Sciences of Amsterdam. Important additions had been made to the Library by purchases during the past year. The books are still suffering from damp, although every method of abating the evil is adopted. But the accommodation altogether is quite unworthy of such a valuable Library. The report was approved of.

MR. JAMES REID, the Treasurer, gave in the following Abstract of the Treasurer's Account for Session 1865-66:—

DR.

1865.—*Nov.* 1.

To Cash in Union Bank of Scotland,	£40	11	2			
,, Cash in Treasurer's hands,	1	1	10½			
				£41	13	0½
,, Entry-money and Dues from 19 new Members at 42s.,	39	18	0			
,, Annual Dues from 5 Original Members at 5s.,	1	5	0			
,, Do. from 1 Original Member for three years,	0	15	0			
,, Do. from 242 Members at 21s.,	254	2	0			
,, Do. from 7 Members for two years,	14	14	0			
				310	14	0
,, Institution of Engineers for Rent,				15	0	0
,, Interest on Bank Account,				2	7	11
,, Taxes recovered from Landlord,				2	7	3
				£372	2	2½
,, Expenses connected with Mr. Bell's Lectures, repaid by Subscriptions by Members of Council, and tickets sold,				30	10	0

CR.

1866.—*Oct.* 31.

By New Books and Binding,				£109	6	6
,, Printing *Proceedings*, Rules, Circulars, &c.,				51	10	0
,, Stationery,				2	17	6
,, Salaries and Wages,				110	5	0
,, Delivering and Posting Circulars,				12	1	6
,, Rent, Insurance, Gas, Coals, and Water,				58	14	5
,, Petty Charges, Taxes, and Postages,				19	5	3¼
,, Balance—In Union Bank of Scotland,	£4	8	9			
,, Treasurer's hands,	3	13	3			
				8	2	0
				£372	2	2¼
,, Expenses connected with Mr. Bell's Lectures,				30	10	0

MR. REID having resigned the office of Treasurer, thanks were voted to him for his services.

The Society then proceeded to the Sixty-fifth Election of Office-bearers, who were appointed as follows, viz.:—

President.

DR. FRANCIS H. THOMSON.

Vice-Presidents.

JAMES BRYCE, M.A., LL.D., F.G.S.
PROFESSOR ROBERT GRANT, M.A., LL.D., F.R.S.

Librarian.

REV. HENRY W. CROSSKEY.

Treasurer.

MR. JAMES MANN, C.A.

Secretary.

MR. WILLIAM KEDDIE.

Other Members of Council.

PROF. W. T. GAIRDNER, M.D.
MR. EDMUND HUNT.
MR. WILLIAM NEILSON.
MR. JAMES KING.
MR. DANIEL MACNEE.
MR. JOHN BURNET.
PROF. THOMAS ANDERSON, M.D.
MR. ALEXANDER HARVEY.
MR. JOHN RAMSAY, Lord Dean of Guild.
PROF. W. J. MACQUORN RANKINE, LL.D., F.R.S.
PROF. ALLEN THOMSON, M.D., F.R.S.
SIR WILLIAM THOMSON, LL.D., F.R.S.

The PRESIDENT exhibited and described a collection of the Poniatowsky Gems; and explained a method contrived by himself for taking impressions in glass from gems.

MR. ROBERT GRAY exhibited specimens of the Red-necked Phalarope, from the West of Scotland, and made a few remarks on the species.

December 5, 1866.—*The* PRESIDENT *in the Chair.*

Mr. James M'Call, V.S., Principal of the Glasgow Veterinary College, and Mr. James Bain, ironmaster, were elected members.

The PRESIDENT mentioned that the Council this evening had had under its consideration the subject of the Exhibition Fund, in consequence of Provost Lumsden having expressed a desire that it should be handed over by the Town Council to the Philosophical Society, provided that the latter could propose any method which would meet the conditions on which the money had been invested in the Corporation. The Council had appointed a committee to consider a plan whereby it was hoped the end in view could be attained.

DR. R. ANGUS SMITH, of Manchester, read a paper "On the Water of the River and Frith of Clyde." The subject gave rise to a discussion, in which Dr. Thomas Anderson, Mr. D. More, Dr. Gairdner, Mr. William West Watson, Dr. Lyon, Mr. Alexander Harvey, &c., took part.

The thanks of the Society, on the motion of the President, were voted to Dr. Smith for his communication.

PROFESSOR YOUNG exhibited specimens of Cestoid Parasites adhering to the Duodenum of a Haddock, and made a few remarks on their habits.

December 19, 1866.—*The* PRESIDENT *in the Chair.*

Mr. George Walker Muir was elected a member of the Society.

The following books were presented to the Library:—

History of the Merchants' House of Glasgow, presented by the Lord Dean of Guild and Directors.

Statistics of New Zealand for 1863-64, 2 volumes, presented by Dr. Bryce.

Transactions of the Historic Society of Lancashire and Cheshire, from the Society.

Transactions of the Botanical Society of Edinburgh, from the Society.

The PRESIDENT read a report from the Council, embodying a pro-

posal to be laid before the Trustees of the Exhibition Fund, with a view to obtaining possession of the money for the use of the Society.

The adoption of the report was moved by the President, seconded by Mr. Alex. Harvey, and approved of. The following were appointed a deputation to wait upon the Trustees and lay before them the proposal contained in the report: viz.,—The Lord Dean of Guild, Dr. Gairdner, Rev. Mr. Crosskey, and Mr. Alex. Harvey.

PROFESSOR YOUNG exhibited vertebræ and fin bones of *Platax Woodwardi*, and teeth of *Rhinoceros Schleiermacheri*(?), *Tapirus priscus*, *Sus*, from the Red Crag, and made some remarks on the climatal changes indicated by the presence of these genera in England.

PROFESSOR YOUNG read a paper "On the Scientific Premonitions of the Ancients."

January 9, 1867.—*The* PRESIDENT *in the Chair.*

Mr. Andrew Bowie, engineer, Mr. John Donald, iron merchant, and Mr. Robert Steel, were elected members.

It was agreed to defer the reading of Mr. Campbell's paper till next meeting, in consequence of the absence of many of the members, from holiday engagements.

January 23, 1867.—DR. BRYCE, *Vice-President, in the Chair.*

Mr. Hugh MacBean, paint manufacturer, and Mr. W. M. Miller, teacher of music, were elected members.

MR. JOHN D. CAMPBELL made a communication "On the Proposed Extension of the Factory Act, and its probable Effect on Juvenile Labour and Education in Glasgow." Mr. George Anderson, Mr. George M'Callum, Mr. John Downie, Mr. John Mayer, and Mr. Crosskey shared in the discussion of the subject.

A paper was read by PROFESSOR W. J. MACQUORN RANKINE "On the phrase '*Potential Energy*,' and on the Definition of Physical Quantities."

MR. MAYER exhibited and described the action of Ansell's Indicators of Fire-damp in Coal Mines.

February 6, 1867.—PROFESSOR GRANT, *Vice-President, in the Chair.*

The following papers were read and illustrated, viz.:—

MR. A. S. HERSCHEL, "On Singing Flames, and the Musical Sounds produced by Heat."

Mr. John Mayer, "On the new Metal, Magnesium: a brief Account of the Method of Extraction, and its useful Applications," illustrated by a series of Specimens lent by the Magnesium Metal Company.

Mr. John Mayer, "On Gale's Protected Gunpowder."

February 20, 1867.—*The* President *in the Chair.*

Mr. Bernhard Von Ammon, Mr. John Slessor, chemist, and Mr. Alexander Steven, jun., sugar merchant, were elected members.

The President reported that the Council are keeping steadily in view the procuring of better accommodation for the Society, and are in hopes of being able before long to bring forward a plan which promises to attain that object for the Philosophical Society in conjunction with several other societies.

The following papers were read:—

Dr. Bryce, "Notice of some New Discoveries in the Geology of Arran."

Professor Young, "Notice of two Instruments for Determining the Variation of Species."

March 6, 1867.—*The* President *in the Chair.*

Sir William Thomson made a communication on the subject of Vortex Atoms, illustrated by experiments.

March 20, 1867.—*The* President *in the Chair.*

Sir William Thomson described the Marine Galvanometer, and other Apparatus for Electrical Tests, used on board the *Great Eastern* in the recent expedition.

Professor Young made a communication "On the Bones of the Face in Vertebrates."

April 3, 1867.—Professor Grant, *Vice-President, in the Chair.*

The following papers were read and illustrated:—

Mr. A. S. Herschel, "On the Heights and other particulars of the late November Meteors."

[redacted]

Mr. John Mayer, "Nitro-Glycerine, or Blasting Oil, the New Explosive: its Manufacture, Properties, and Uses."

Mr. Mayer exhibited two forms of Ansell's Fire-Damp Indicators, in practical operation.

April 17, 1867.—*The* President *in the Chair.*

Mr. Gorman read a paper "On Results obtained from Heating Scrap Iron in the Heat-Restoring Gas Furnace, and in the Furnaces commonly employed for that purpose, with Comparisons and Observations on the Quality of the Iron Welded, and on the Flame of the Common and Gas Furnaces."

Dr. Allen Thomson gave an account of recent Discoveries in the Structure of the Internal Ear, with Microscopical and other illustrations.

The President then announced the close of the Session.

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

SIXTY-SIXTH SESSION.

Opening Address by the PRESIDENT, DR. FRANCIS H. THOMSON.

November 6, 1867.

GENTLEMEN,—It gives me great pleasure to see present so many of the well-known and welcome faces which have for many years appeared to inaugurate the Session of the Philosophical Society. We are now in our Sixty-sixth Session; and although we have to mourn the loss of some distinguished men connected with us, I am glad to see around me many of the hard workers in science; and I sincerely trust that, looking back to the labours and success of last season, we may hope that the present may be fruitful in papers upon the varied subjects which in time past have been so well treated in this room.

Since we separated in April last, one has departed from amongst us who has left a name deeply revered by all who had the pleasure of his acquaintance—I might almost say, a man universally beloved. I allude to the late Mr. Walter Crum, who died on the 5th May of this year.

Mr. Crum was born in 1796, and was reared and educated under the shadow of our venerable University. In 1818 and 1819, Mr. Crum was a distinguished student of practical chemistry under the late Dr. Thomas Thomson. Of his fellow-workers, amongst whom were the late Mr. Dunlop, of Clyde, Mr. Knott, an American, and Mr. Angus, sugar refiner, Greenock, only two now remain—namely, Mr. C. J. Tennant, of St. Rollox, and our own well-beloved Alexander Harvey, who was the assistant in the class.

From this period Mr. Crum devoted his time and energies to the cultivation of that scientific knowledge which gained for the house of which he was the head, a place in the first rank, and made it famous for the high character of its transactions and products. As a merchant, he was eminently distinguished for his quick and ready judgment, his

practical scientific knowledge of his own special department, and above all, that true, one might almost say, stern, standard of honourable dealing which was his leading characteristic.

So far back as 1823, Mr. Crum published in the *Annals of Philosophy* the results of a long series of experiments upon Indigo, the title of the essay being, "Experiments and Observations on Indigo, and on Certain Substances which are produced from it by means of Sulphuric Acid." This memoir embraced views and facts regarding the chemical peculiarities of this material which are now recognized as the standard, having never, in all these years of progress and scientific investigation, been overturned. The paper was very elaborate, showing how minute were the experiments and broad the principles adopted; in fact, so much importance was attached to his labours, as bearing upon manufacturing interests, that an almost European reputation was the result. Thus he was brought much in contact with the best chemists of the day, enjoying the intimate friendship, amongst many others, of Liebig and the much-lamented Faraday.

Mr. Crum became a member of this Society in 1834, and succeeded Dr. Thomas Thomson as President in 1852. During all the intermediate years he was an active and industrious member, contributing regularly to the *Transactions*, and serving faithfully as Councillor and Vice-President.

Amongst the many papers written by him, included in our *Transactions*, the following may be quoted as the most important:—"On Chlorinometry, and a new Mode of Testing weak Solutions of Bleaching Powder;" and the same year an interesting memoir "On the Manner in which Cotton unites with Colouring Matter." Again, we find him, in another department of scientific investigation, treating of the supposed influence of the Moon upon the Weather. In 1845 he was peculiarly industrious in the interests of the Society, as evinced by three important papers—the first, "Upon the Action of Bleaching Powder on the Salts of Copper and Lead;" the second, "On a Method for the Analysis of Bodies containing Nitric Acid, and Unexplosive Cotton;" and again, "On the Artificial Production of the Potato Disease." Afterwards he furnished a paper "On a Peculiar Kind of Cotton which is incapable of being Dyed;" and in 1852, when he became President, he delivered an address "On the Life and Labours of Dr. Thomas Thomson," who so long and worthily presided over us. Many here must recollect Mr. Crum's discourse on that occasion, and his judicious estimate of the genius and acquirements of our illustrious President, whom he had known all his life; and who, therefore, so well fitted to do justice to his memory?

About the last communication from Mr. Crum was a short memoir "On the Stalactitic Sulphate of Barytes found in Derbyshire." His only separate publication was a little work which appeared in 1830, *On the Primitive Colours.* We all remember Mr. Crum in this Chair, and retain pleasant recollections of his calm and philosophic views on the various papers brought before the Society. He made no pretensions to oratory, but his opinions were always weighty and to the point; and whilst we deeply mourn the loss of one personally dear to most of us, our regrets are tempered by the knowledge that he well fulfilled his mission in all the relations of life. I feel convinced he has not left behind him one enemy; and only pleasing memories of his worth and usefulness remain.

I may be excused for mentioning a touching incident which came under my notice at Mr. Crum's funeral, and perhaps was only noticed by myself. An old man—one of his veteran workmen, who had evidently known him from childhood—stood beside the grave, the tears coursing down his furrowed cheeks; and when "earth to earth" testified that all was over, he ejaculated in an undertone, "Poor, poor Walter!"

I cannot conclude this imperfect sketch of our late friend without referring to his connection with one of our great educational institutes—the Andersonian. For some years he held the office of Chairman, and to him is due its present prosperity in many ways; for, along with Mr. Smith of Jordanhill and others, he carried out and perfected the various improvements and additions which now render the Andersonian Institution one of the most efficient of its kind in Scotland. One of his pet schemes was the remodelling of the museum; and day by day was Mr. Crum to be found there, along with Dr. Scouler, re-arranging the collection. The museum is now consequently of a very high order, and, from its admirable classification, must prove of great educational value. I may also mention that Mr. Crum took a warm interest in the success of the Glasgow Athenæum, and was one of its earliest and most enthusiastic workers. He came on the field at a time when his influence was of the greatest importance, and I am told by my friend Mr. Provan, who was the originator of the scheme, that but for Mr. Crum the institution could not have gone on.

The last time I had the pleasure of seeing Mr. Crum was about three weeks before his death, when he delivered to the Andersonian Trustees a short eulogium upon his late friend, Mr. Smith of Jordanhill, who, gathered to his fathers at a ripe age, was deeply mourned by one who had been his loving companion through much of his long life. Little did we think, when listening to his sorrowful words, that they

were to be his last in this room; but even then the bolt had sped which was to deprive us of our dear and estimable friend. I cannot more appropriately close these sad recollections than by a quotation from an address by his friend, the Rev. George Campbell of Eastwood, which gives a touching description of the sunset of Mr. Crum's kindly and virtuous life :—

"My last remembrances of him are connected with a Sabbath evening spent with him and his family a few weeks ago, when I was led, as I then thought, by a casual circumstance, but as I now believe, by Divine guidance, to select as the subject of my address to the neighbouring families gathered in the house, the sublime death of the patriarch Moses. And while greatly struck with his whole demeanour that evening—with the gentleness, the grace, the humility, and meekness of his spirit—I cannot but think now that the profound interest which he felt in the subject, and expressed to me in the warmest language, was due not only to the veneration which he said that he had always felt for the lawgiver, as one of the grandest characters in history, but to a feeling that the good Shepherd of Israel was even then leading him to his rest, and was soon to lay him to sleep in His loving arms. It has proved so; and we are bound to thank God for him—for his talents so faithfully cultivated and so actively and usefully employed; for a life so well spent and so peacefully closed, 'coming to his grave in a full age, like as a shock of corn cometh in his season.' May God be pleased to grant His consolations to a family so sorely bereaved; and, while taking away the useful and the good, may He raise up others to walk in their steps; and grant to all of us that we may each in our sphere fulfil the work and purposes of the Great Master, so that at His coming we may be found as good and faithful servants, and be prepared to receive Him, and to enter into the joy of our Lord, to reign with Him in that blessed kingdom to which He is so quickly gathering home His precious ones from the storm and trouble impending upon the earth! 'The righteous perisheth, and no man layeth it to heart; and merciful men are taken away, none considering that the righteous are taken away from the evil to come. He shall enter into peace: they shall rest in their beds, each one walking in his uprightness.'"

When I had the pleasure of addressing you at the opening of our last Session, it was on the subject of the Workshops of Glasgow. The matter that might be included under this head is inexhaustible; and having so far considered iron manufactories, and described some of our more important local works, perhaps I may now be permitted to take

up for a short time, without reference to locality or department, some other works interesting from their novelty or their important illustrations of applied science.

When I had the honour of being in Dundee as one of the deputies of this Society to the late meeting of the British Association, I had an opportunity of examining a work which to me was highly interesting. It is that of Messrs. Cox Brothers, Camperdown Jute Works, Lochee, which is the largest in the country, except that of Messrs. Baxter, who employ a few more horse-power. The manufacture is one of comparative novelty; and being already carried to a great and successful extent in Dundee, a notice of it may not be unacceptable to the Society.

Jute is the fibre yielded by two plants named *Corchorus olitorius* and *Corchorus capsularis*, both products of Bengal. It is of this material that the "gunny" bags of the natives are made. It is cheaper than "Gunn," which is one of the strongest of the Indian fibres, and comes into competition with flax, codilla, &c., especially in the imitations of Brussels and other carpets for which the Dundee manufacturers have now become famous. From its tendency to snap after immersion in water, jute is not so well suited for the manufacture of rope and cordage. The importance of the material may be estimated by the returns of the Indian exports. In 1850 England imported 391,098 cwt., of the estimated value of £88,989; this gradually increased till 1865, when the imports rose to 2,108,942 cwt., valued at £1,774,992. A curious anomaly took place in 1859, when 317,890 cwt. was imported, valued at £525,099, whilst the year previous had produced 788,820 cwt., valued at £303,292. This inequality arose in some measure from the Crimean war, or from some peculiar condition of matters between the exporters and manufacturers; but be this as it may, it resulted in many instances in large fortunes being realized to the Dundee manufacturers.

The number of spindles acting in Dundee may be computed at about 40,000, the annual consumption of jute being about 50,000 tons. One might infer from these figures that jute and its manufacture must have been long understood, and the different processes of its manufacture thoroughly defined. On referring, however, to recent authorities, little or no mention is made of it, the trade having only grown up to its present dimensions within the last fifteen years; the manufacturers, meanwhile, not much caring to make known the processes employed, by opening their works for public inspection.

Dr. J. Forbes Royle, writing in 1855, gives an account of the history and culture of jute, in his able work *On the Fibrous Plants of India*. The subject in itself is an important one, but too extensive to be

entered upon fully in a notice of this scope. I shall therefore content myself by briefly describing what I had the privilege of witnessing in Messrs. Cox's establishment.

The works are situated at Lochee, about three miles from Dundee, and cover upwards of twenty acres. Their arrangements include every appliance that is new. The facilities for speedy production are much enhanced by certain special original movements in the machinery, invented and adapted for this house, and known only within the walls of the manufactory.

On first entering, you are struck with the clean and orderly appearance of everything and every person you meet. The utmost regularity prevails, and every department is separated and defined. The buildings are from one to five storeys high, and are arranged in regular streets. Everything required in the work is manufactured within the walls, there being special departments for moulding, casting iron and brass, gas-fitting, plumber work, mason work, painting, joiners, and certain of the engines are even manufactured by themselves,—in fact, with the exception of the large producing engines, the entire apparatus is made on the premises. The workers number over 4,000, and these include spinners, weavers, dyers, bleachers, calenderers, lappers, packers, mechanics, blacksmiths, moulders, masons, joiners, &c. The quantity of material consumed is about 40 tons daily: the consumption of coals is about equal. The thread produced in twelve months is calculated at nearly 52,000,000 of yards. The chimney, which is 302 feet from the foundation, and highly ornamental, is in some respects unequalled in any part of the kingdom; and I shall here introduce one or two facts regarding it, which have been communicated to me by my friend Mr. Cox.

The erection was commenced in June, 1865, and finished in August, 1866, although the building had to be stopped during four of the winter months, when at the height of 180 feet from the surface of the ground, and covered over with a roof to protect it from the weather. The height above the surface of the ground is 282 feet; the foundation is 35 feet square, being a single course of ashlar 18 inches thick, laid on the boulder clay about 22 feet below the surface of the ground; the weight is distributed over the whole, except a portion in the centre 10 feet diameter, giving an area of 1,147 square feet to support the structure, the entire weight of which cannot be short of about 5,400 tons, giving a pressure of nearly $3\frac{3}{4}$ tons per square foot. It has not subsided in the slightest degree. The materials were taken up outside the chimney, by means of a hoist driven by a portable steam engine, and the whole structure was completed without any accident. The

work was not contracted for, but erected under the supervision and at the risk of the proprietors. At present there are twenty-two furnaces working into the chimney; that number, however, will be doubled in a short time, as all the other furnaces in the works are intended to be led into it. The chimney is removed from the boilers to a distance of 75 yards, and connected by a large flue of 50 feet area. A draught of $1\frac{9}{10}$ inch, measured by the common gas pressure gauge, is realized, being nearly double that of many effective chimneys. There is access to the balcony by means of an iron ladder built in one of the corners, which are formed by the outside walls of the chimney being square, whilst those of the smoke-shaft inside are circular. You will observe from the drawing that the form of the chimney is square up to a height of 230 feet, where the heavy cornice or balcony occurs; above this to the top is octagonal for other 50 feet, finished with a moulded cope. It is built in very large panels, in various designs, which are composed of parti-coloured bricks. The base, in outward appearance, is of droved ashlar, surmounted by a massive moulding, above which is the first panel, the styles being checkered red and white, with a geometrical figure in the centre of each panel. Above this is a base moulding, out of which spring the pilasters or styles of the next panel, which joins the shaft of the tower, and is 147 feet high. It is relieved by loop-holes in the centre, and a blank clock face on the top of the panel. The following are some of the principal dimensions:—

Foundation, .	35 feet square.			
At surface of ground, .	30 feet.	square walls,	6 feet	thick.
At top of first panel, .	24 ,,	,,	3 ,, 3 in.	,,
At top of main panel, .	21 ,, 3 in.	,,	2 ,, 5 ,,	,,
At balcony, . . .	20 ,, 3 ,,	,,	2 ,, 0½ ,,	,,
At top of chimney, .	19 ,,	,,	1 ,, 6 ,,	,,
Diameter of smoke shaft at bottom, . .	14 ,, 6 in.	,,	1 ,, 6 ,,	,,
Diameter of smoke shaft at top, . . .	13 ,, 6 ,,	,,	0 ,, 9 ,,	,,

The jute is brought direct to Dundee in vessels belonging to the firm, and conveyed to Lochee by a line of railway, from which there is a special siding running into the warehouses. The bales weigh each about 301 lbs., and measure about 10 cubic feet. These bales are removed as required, along accommodation rails, to the room where the first operation commences. Here the ropes which tie the bales are cut, and the bundles, or strikes, as they are denominated, of jute, taken to the softening machines. After passing this ordeal, the material is cut into certain lengths, the original length of the fibre being from 6

to 15 feet. After being slightly saturated with oil, it is selected or divided into qualities, one to be heckled and the other teased into shorter lengths. The heckled kind produces the line or long staple yarn, and the teased the short staple or tow yarn. From the cutting machine the portion to be heckled is taken to the room set apart for this purpose, where it passes through the heckling machine, by which it is again divided into two qualities—the longs and the shorts. The latter is mixed amongst the teased portion of the bale, and built into a large square stack like a hayrick, termed the batch. Thence it is taken to the second flat to the breaking engine, and from it is discharged like a long ribbon, termed a sliver, into a tin can about four feet deep. A number of these cans containing the sliver are placed in front of the balling machine, which conveys the sliver round a roller like a bobbin. When full, these bobbins are removed to the carding engine, which again discharges the material in the shape of a ribbon; and from this it is transferred to the drawing machine, which reproduces the sliver, but in a much finer state. The next machine is the roving frame, which emits the sliver like a small rope slightly twisted, and round a bobbin called the rove bobbin. The bobbins are now conveyed to the third and fourth flats by the elevators or hoists, to be spun into yarn. The bobbins containing the yarns are then taken to the fifth or upper flat, to be reeled into the hank and put into bundles. Thus is finished the preparation and spinning of the tow.

We shall now go back to the heckling rooms, and follow the longs from that department to the yarn bundles. After passing through the heckling machine, the longs, or heckled portion of the fibre, are carted to the second floor, or preparing flat, to the first machine, called the spreading machine. This delivers the material into a can or sliver, from which it is moved forward to the next or drawing machine—then to the roving, which discharges on the rove bobbin, twisting slightly as in the tow spinning, and so on as before.

The yarns when finished are now moved to the warehouse for sale, or to the washing, bleaching, or dye houses, there to be prepared for the weaving department. In the washing house the yarn is prepared for the production of the common fabrics, which are kept as near the natural colour as possible. Those intended for the production of the beautiful imitation carpets for which the house is celebrated, are bleached by the usual chlorine process. The yarns are partially dried by the hydro-extractor, and from thence conveyed to a series of ovens or boxes, from whence they come out in the space of a few minutes thoroughly dried. The warehouse now receives both descriptions of yarns to be given out as required—either for sale or for use in the

works. For these purposes upwards of forty horses are constantly required.

The process of weaving is necessarily much the same as in all factories for the production of canvas or carpets, and need not be described here, except that, as before stated, many original and beautiful simplifications have been adapted and utilized by the head partner of the firm.

During the American war large fortunes were made by the Dundee manufacturers, for jute canvas was in many cases used where flax could not be got. Gradually, however, the trade has resolved itself very much into the production of the lighter descriptions of canvas, used for sugar-bags and cotton-bale coverings, which are exported to America and the colonies.

The Messrs. Cox deserve much credit for the beauty, regularity, and economy of their works; and it is matter of admiration that this success is mainly due to the unaided and indomitable industry of the four brothers, who began and have carried out the undertaking to its present proportions.

Jute manufacture has been introduced into Glasgow within the last two years, in the form of a limited company; and if one may judge of the success of the undertaking by their published statements, there cannot be a doubt of its becoming a leading manufacture of this city.

The subject of mineral and manufactured oils has now become one of great commercial importance, and it may not be out of place to spend a short time in considering its bearing on the future, and tracing the rise and progress of this new phase of scientific industry.

In 1847, Dr. Lyon Playfair turned his attention to the artificial production of petroleum, or rock oil; but we owe the working out of the problem to our distinguished townsman, Mr. Young, who, after many years of patient and industrious experiments, elicited results astonishing from their magnitude and general utility.

It is not my intention to trace the various struggles which have eventuated in the well-deserved success of Mr. Young, or to enter upon a description of his process of production; but it may be interesting to you if I put, in a condensed form, some statistics showing the importance of the trade, and add some information as to the qualities of this oil as a fuel. I would refer those interested in the working of Mr. Young's process to several well-informed articles in the *Herald* and *Morning Journal* for the month of March, 1866, minutely describing Mr. Young's works, as also giving full credit to his individual exertions.

Although the introduction of the mineral oils of America has caused not a little damage to the producers in this country, and reduced the

prices even below working expenses, insomuch that, with very few exceptions, our Scotch makers have been virtually put out of the field; yet, as time goes on, the sources in America will, in all probability, be lessened, if not exhausted, whilst our shale is comparatively inexhaustible. In the present state of matters it is only such huge concerns as the Mineral Oil Co. (Limited), backed as it is by unlimited capital, that can compete with the low-priced oils which flood this country from abroad.

The consumption of these oils in all probability must very much increase; for we are yet in the infancy of their utilization. Add to this the increasing demand in America, and all over Europe, for the better class of refined oils—a demand which, for the present, is throwing into this country, to compete with our own products, much of the inferior inflammable oils, at such prices as to render competition impossible.

Already the supply of the mineral oils of America has begun to fall off. In 1866 the production outstripped the consumption by 6,000 barrels of crude oil per day; whereas in 1867 the consumption exceeds the production by 7,000 barrels per day. In further proof of the importance of this material, it may be stated that during the last five years the annual yield of the American oil springs has been upwards of 500,000,000 gallons, averaging from 1s. 5d. to 1s. 6d. per gallon.

Of late much attention has been drawn to the danger arising from the importation of the low class and highly inflammable mineral oils, and with regard to which the Americans have found it necessary to pass an act of a very stringent nature, as follows :—" And be it further enacted, that no person shall mix for sale naphtha and illuminating oils, or shall knowingly sell, or keep for sale, or offer for sale, oil made from petroleum, for illuminating purposes, at less temperature or fire test than 110° Fahr.; and any person so doing shall be held guilty of a misdemeanour, and on conviction thereof by indictment in any court of the United States, having competent jurisdiction, shall be punished by fine of not less than 100 dollars or more than 500 dollars, and by imprisonment for a term of not less than six months or more than three years."

One result of this enactment has been to throw into this country those base unsaleable oils which are useless in America; and if we would avoid such a calamity as lately occurred at Bordeaux, when more than thirty persons were blown up by an oil explosion, with serious loss of life, we ought to urge upon Government the necessity of a stern restrictive enactment; for if we except the abortive act of 1862, fixing the firing point at 100°, the honest trader in this country is totally unprotected. I believe that Government have had their atten-

tion drawn to this point, and it is hoped that ere long decided action will be taken.

The oils made in this country, as also the American refined oils, invariably stand the test of 110°, and if we are to ensure safety, we cannot be satisfied with less. Insurance Companies should see to this as a matter of self-protection; for in innumerable instances unknown risks are ignorantly taken, where false representations have been used as to quality, and when explosive oils have been substituted; and in this country, not less than on the other side of the Atlantic, it is well understood that a careful inspection of storage and sale of petroleum is absolutely required.

A word or two further in illustration of the extent to which oil refining has been carried in this country. The refineries of Great Britain turn out something like 500,000 barrels in a year. In Scotland last year we had twelve refineries whose production was 5,000 barrels weekly, but in 1867 the output amounted to only 1,500 barrels per week. Wales at the same date turned out 6,000 weekly, but reduced it to 1,000; and at this date, as before stated, most of the Scotch refineries are at a standstill, waiting the tide of events. Unfortunately, the parties who invested capital may not derive the benefit of any prospective prosperity which may arise; but I believe the day is not far distant when, Phœnix-like, they will revive and lend their aid in fulfilling the destiny of this material, which ultimately has a mighty service to perform in supplementing our failing supplies of fuel. But before going into this question, we may refer shortly to one of the results of the distillation of these oils—that of paraffine, the solidified hydro-carbon—and here again we owe to Mr. Young the practical illustration of this comparatively new illuminating power.

So far back as 1830, Baron Reichenbach exhibited to the German Association of Naturalists at Hamburg the first specimen of paraffine, and for some years he continued his experiments upon various vegetable oils, but found the quantity so small as to be non-remunerative. In the Exhibition of 1851, one paraffine candle was exhibited. In 1862 a pretty large block was shown; but Mr. Young, who was determined not to do things by halves, produced in the Dublin Exhibition a solid block weighing upwards of half a ton; and its purity was demonstrated by its colourless, inodorous, tasteless, and beautifully translucent appearance.

Paraffine candles now compete with the lowest class of tallow candles; and this may be easily understood from the fact that for each ton of oil refined, 32 lbs. of paraffine is obtained. In almost every village paraffine candles can now be bought.

Mr. Edward Franklin, in a lecture delivered at the Royal Institution, "On Artificial Illumination," states, that "illuminating equivalents, or the quantities of the different illuminating materials necessary to produce the same amount of light," are as follows:—

Young's paraffine oil, ...	1 gallon.
American petroleum, No. 1, . . .	1·26 „
American petroleum, No. 2, . . .	1·30 „
Paraffine candles, ...	18·6 lbs.
Sperm, ...	22·9 „
Wax, ...	26·4 „
Stearic, ...	27·6 „
Composite, ...	29·5 „
Tallow, ...	36 „

From this statement it is evident that 1 gallon of Young's oil is equal to from 1¼ to nearly 1⅓ gallons of American petroleum oil, and an interesting experiment was made upon a sample of Young's paraffine oil and two samples of American petroleum oil, which showed that at a temperature of 120° the petroleum oil formed explosive mixtures, while the paraffine oil did not.

A curious statistical return, connected with the utilization of these oils for illuminating purposes, occurs in the report connected with the Industrial History of Birmingham, prepared by the Local Industries Committee of the British Association in Birmingham, 1865.

"In 1860 a lamp manufacturer produced in one year 247,431 lamps for the consumption of oil manufactured by Mr. Young. In 1861 the same manufacturer was producing at the rate of 1,200 per day, or 375,000 per annum. At the first introduction of paraffine oil these lamps were produced by Scotch houses engaged in the brass foundry trade; but in 1861 the trade was introduced into Birmingham by four lamp manufacturers. To these a new establishment was added, and the smaller manufacturers turned their attention to the production of burners. The Birmingham production of paraffine burners reaches 500,000 annually." The report gives further particulars of the manufacture which it is unnecessary to quote.

HYDROCARBONS AS FUEL.

Of late attention has been drawn to the probability of utilizing the hydrocarbons as fuel, and as this is a matter of great practical moment, I shall mention some facts, to illustrate to what extent experiments have been made, and with what results.

Government in 1856 ordered certain experiments at Woolwich Dockyard, with the view of testing the value of petroleum and shale oil as

a substitute for coal in raising steam in marine boilers. The experiments were carried out extensively by Mr. Richardson, upon American petroleum, English coal oil and shale oil, Burslem oil, and Torbane Hill mineral oil. Fifteen separate experiments were made, the duration of which varied from 2 hours 25 minutes to 10 hours 20 minutes. The total weight of oil used for getting up steam was 499 lbs., and 4,755 for the whole experiments.

Taking the average of the whole experiments, it appears that 13·2 lbs. of water were evaporated per lb. of oil. The lowest results of the series were those given on two consecutive days by a mixture of American oil and coal oil once run, burned in three furnaces. On the first day 7·77 lbs. of water were evaporated per lb. of this mixture, and on the second day 7·14 lbs. of water per lb., a result lower than that obtained from coal burned in the ordinary way.

The result of these experiments was not very satisfactory, the combustion having been imperfect. The report gives a detailed description of each experiment, which, although interesting enough in themselves, do not seem to have been very successful; but, on the whole, the experimenter seems to give the preference to the Torbane Hill mineral oil and Burslem oil, which evaporated the water at the rate of 18·38 lbs. to the lb. of oil. The smoke was very moderate, and the tubes at the conclusion of each experiment were tolerably clean.

The report concludes that the experiments, so far as they have gone, may be regarded as of considerable value, as showing the great evaporative power of these oils, and the practicability of their utilization.

In an economic point of view there may be some doubts of the value of this application. At the present price of petroleum oils, it is not easy to suppose that any considerable saving could be effected; and so far Mr. Richardson's experiments, which, however, are only initiatory, do not promise much.

But various parties are now turning their attention to this important subject, and certain experiments which were instituted by Mr. Barff have resulted in the formation of a limited company in London, called Sim and Barff's Patent Mineral Oil Steam Fuel Company; and they introduce themselves by stating that they have taken out a patent for utilizing the lighting and heating properties of petroleum, tar, oil, naphthaline, and other heavy inexplosive hydrocarbons that have hitherto been comparatively useless on account of the difficulty experienced in combining with them, at the burning point, sufficient air to cause perfect combustion.

Some idea may be formed of the commercial value of the lighting and heating properties of these heavy oils, from their possessing three

times the evaporating power of coal, requiring much less space for stowage, and thus effecting a great saving in labour. On this account Messrs. Sim and Barff affirm that these oils are doubtless destined to form the marine steam fuel of the future. They add, that by their process no alterations of existing furnace arrangements are required.

In the *Times* of 28th January, 1867, there was an elaborate report by Professor Bloxham upon experiments which were made at Messrs. Jackson and Watkins', Millwall, by the patentees; and although too extended to be more than alluded to in this notice, the results are satisfactory. He concludes by saying, "The boiler tested at Millwall was a return flue boiler, and although of unfavourable proportions, some good results were obtained. With the boiler three parts filled, the pressure-gauge indicated 25 lbs.; in three minutes it was 30 lbs., the safety-valve being eased at this pressure. With all these disadvantages to contend with, the gentlemen present expressed their complete satisfaction with the results; and as a company has been already formed to work this patent, one looks with interest to the result of their future trials. The simplicity and easy adaptability of the apparatus to existing boilers commends itself at once to the attentive consideration of the public. A vessel of the size of the 'Persia' would save alone by extra cargo, in using this fuel, for a return voyage to New York, no less a sum than £3,000. By this method the rate of evaporation actually obtained in a boiler very unsuitable for the purpose has been the highest ever known—22 lbs. of water to 1 lb. of oil, or in the proportion of almost 4 to 1 against ordinary steam coal; thus, a saving of two-thirds of the space on board ships now occupied for the stowage of fuel is effected, and made available for carrying cargo; so that although the actual cost of the fuel is greater, weight for weight, than that of coal, still the increased cargo-carrying capacity causes such diminution in the cost of running a steamer as to leave a large margin for profits. For naval purposes the importance of adopting this method cannot be overrated. It is well known that many of our iron-clads, with their complete armament on board, are able to carry only three or four days' supply of coal. In the application of this method to the manufacture of iron, it may be stated that some of the most rapid meltings ever effected have been obtained by the use of the patented method—thus showing its high efficiency under another aspect."

The same gentlemen have also practically carried out a patent by J. Kidd for using the dead oil of tar, or any dead oil, for carburating the common coal gas. Mr. Barff writes me, stating that the gas engineer of the London and North-Western has reported upon it, and

the company is to have the lighting of the departure platform of the Euston Station. A train of twelve carriages is at present running on the North-Western between Broad Street and Chalk Farm, six of them lighted by Sim and Barff's process, the others working a patent by Professor Blagden. The latter gentleman uses, however, an explosive oil, which passes over the bag in which the gas is kept in the guard's van, whilst the others use essentially dead oils, which for safety and economy seem to carry the day.

It may be interesting to devote a minute or two to the peculiar qualities of these oils, in combination with carbureted hydrogen, for lighting purposes, and to note some of the results which emerge in an economic point of view. The patentees state that 1 foot of coal gas will absorb from 20 to 30 grains of the oil, by which its illuminating power is increased upwards of 400 per cent. Thus, 1,000 cubic feet of coal gas, costing 4s. 6d., will absorb 5 pints of the prepared oil, costing about 11d.—total cost, say 5s. 6d. It will then give out an illuminating power equal to 5,000 cubic feet of gas, which costs 22s. 6d.

In the metropolis there are 45,000 public lamps, on which an immense yearly saving might be effected by the application of Kidd's process, as employed by Messrs. Sim and Barff. In proof of this, each street lamp in London and its vicinity is computed to consume 5 cubic feet of gas per hour, the average time of burning being twelve out of the twenty-four hours. Thus each lamp consumes 60 cubic feet of gas per night, which is equivalent to 22,000 feet per annum. The ordinary cost of gas for street lamps being, as before stated, 4s. 6d. per 1,000 cubic feet, and the average annual consumption being 22,000 cubic feet, this brings the annual cost of each street lamp to £4, 19s.; whilst by the application of the carburator the expense is reduced to £2, 7s., effecting a saving on each lamp of £2, 12s., and giving a light of 400 per cent. greater.

Again, Messrs. George Miller and Co., Rumford Street, have interested themselves to some extent in the question of the application of oil as fuel, and have now working a large furnace used in heating a steam boiler, heated entirely by the application of tar oil, the refuse of gas works, and steam. The simplicity of their arrangements is certainly very remarkable, and to my idea solves the question of the absolute combustion of the hydrocarbon liquid. The difficulty of burning these oils *simpliciter* has hitherto arisen from an accumulation of coke, arising from the unconsumed carbon accumulating at the bottom of the furnace. In this furnace all is consumed, and the interior is perfectly pure and white.

In carrying out this arrangement the bars of the old furnace had

been removed, and the space floored with fire-bricks. In the centre a pier was raised, rounded on the edges so as to allow an easy play of the flame. The ordinary door is still used, but over it is an iron plate, through which two nozzles are introduced, projecting about 6 inches into the furnace. These nozzles are kneed continuations of two pipes coming from the tar reservoir, each pipe having a stop-cock to regulate the supply. At the knee of each tube steam is introduced, which meets the tar oil as it descends. One or both of the nozzles may be brought into play. When I saw the furnace working, the full power was on. When an additional supply of oxygen is required, the door is slightly opened.

Messrs. Miller state that two tons of tar oil, at 1d. per gallon, give out an amount of heat equal to three tons of the best coal; and that, in an economic point of view, at this rate, and with the oil at their hand, the expense is much the same. Their works, being contiguous to the gas manufactory, gives them facilities as to the supply of the tar liquid which are almost unbounded. But even taking all the difficulties into consideration, when we calculate the magnitude of the saving to be derived by sea-going steamers, the importance of such an application cannot be over-estimated.

Many people have been working at this question; and, amongst others, Mr. Swan, of Edinburgh, has taken out a patent for a combination of hot air and petroleum, to be used in the smelting and forging of iron, and is about to carry out extensive experiments in the blast furnace. His experiments are interesting, as showing the importance of steam or air, in combination with these oils, to effect perfect combustion. He states that, in using the oil alone, a thick deposit was thrown down, and little heat obtained; but when the hot air was used in combination, little or no smoke was evolved, and an intense heat was got up at once.

Again, Sir James Simpson, of Edinburgh, lately applied for a patent for improvements in the utilizing of mineral oil and other oils for the production of heat and for illuminating purposes. He claims the use of either steam or air forced through tubes, by blowing apparatus, the object being to break the jet of oil into minute spray, to facilitate its ignition. This patent has not been proceeded with, in consequence, I presume, of the other patentees having forestalled him.

The exact amount of saving, and the quantity of steam or air required for absolute combustion, has not yet been quite ascertained; but much has been done, proving that the right path has been entered upon; and the subject is in itself of sufficient importance to invite our attention.

IRON.

I should now wish to say a few words upon the manufacture of iron and steel, and note some of the improvements which have been brought up during the past year.

In my address of last season I drew your attention to this subject in connection with Bessemer's process, which, with the exception of the invention of the hot blast, has been the most important addition to the improvements of this material in our day. This branch of industry has in its history three defined stages—viz., the invention of the puddling process, in 1784, by Henry Cort; the hot blast, by James Beaumont Neilson, in 1828; and the Bessemer patents in 1855 and 1862. Each process has its own peculiar interest, but in the special department of steel-making, which up to this date was very tedious, no great improvement had taken place till Bessemer surprised the world with his simple and brilliant idea. It may be well to refresh our memories with the principle of this beautiful operation, and this may be done in a few words.

The pig iron is melted in a common reverberatory furnace and run into the converting vessel; in this it is exposed to a blast of air so as to exhaust the carbon; a quantity of speigle iron, which is a compound of iron and manganese, is then run amongst the melted mass,—this contains a fixed quantity of carbon; the blast is again put on for a certain number of minutes, the progress being indicated by the colour of the flame issuing from the mouth of the convertor, which is now slowly moved by hydraulic power, turned so as to allow the melted mass to escape into a large pot lined with fire-clay, having a moveable plug in the bottom, which allows the metal to run into the moulds, the pot having been previously raised so as to be directly over it. In the production of steel by this process we ascertain that the fuel required is below one quarter of that formerly used in the old methods —a most important item in our calculations, when we look to our future as regards coal.

The importance of this invention can hardly be illustrated without quoting an approximate estimate of the present rate of production of Bessemer steel, and of the existing productive power of plant in Europe by this process. According to Professor Turner's estimate, we have fifty-two convertors in operation, capable of producing 6,000 tons of steel per week. Prussia has 24, with a capacity of 1,460 tons; France 12, producing 880; Austria, with 14, can turn out 650; Sweden, with 15, has a productive capability of 530 tons: there are 2 convertors in Belgium, 2 more in Italy, also 1 or 2 in Russia, making the total

power of production of Bessemer steel now available for Europe equal to about 475,000 tons per annum. To this must be added America, with at least 30,000 tons, giving a total of half a million. In a paper read before the British Association at Dundee by Mr. Ferdinand Kohn, he states that of course the amount produced for this year up to the present date does not exceed 200,000 tons, the quantity for the whole of last year being 362,000; yet in his estimate he quotes England's power of production as being twice as great as that of all the rest of the world, and its actual manufacture of steel of all kinds follows the same proportion. The Barrow Hæmatite Steel Works are the largest in this country, and the greatest but one in the world; but great as they are at present, the company intend to double their capacity. They have now in action ten Bessemer steel convertors, four capable of holding five tons each. They are arranged in two pairs, each pair occupying a separate pit, whilst the other six, being constructed to contain a charge of seven tons, are placed in groups of three. If we consider each vessel as capable of turning out four charges per day, and suppose that all are working, the production would be something like a thousand tons per week. They can produce an ingot, by their accumulated power, of 21 tons—a weight which no establishment in England can equal or exceed.

Mr. Kohn goes on to say that the finest specimens of Bessemer steel were those from Austria and Sweden; but no great credit to them, as their materials are the purest in Europe. The ores, which are pure hæmatite, are smelted with charcoal, and the liquid iron is run into the convertor direct from the blast furnace—no speigleisen is required, but simply a small quantity of liquid iron from the same furnace, used for adding the requisite carbon after the complete decarburization of each charge.

Mr. Bessemer, in his later patents, has much modified his process, in order to make it suit the inferior qualities of iron existing in this country; and that he has been successful in this peculiar treatment may be inferred from the fact, that English hæmatite iron is exported in large quantities to the Continent, and used by steel-makers in France and Germany in the manufacture of Bessemer steel, in preference to iron made in their own localities. Great as has been the success of Mr. Bessemer, he does not stand still: he is now carrying on extensive experiments, more especially in connection with rolling, or otherwise shaping, the cast-steel in a liquid state under pressure.

Mr. Whitworth, Manchester, has tried hydrostatic pressure for this purpose; and however great the difficulties of managing large masses, this peculiar application of applied science may be expected to have a

great future; for after the wonders we have seen as to the effect of Bessemer's process, one might even fancy a time, not far distant, when steel will be shaped direct, without the trouble and cost of manipulation now in use.

It is evident that we are on the verge of great discoveries, not only in simplifying the production of steel of a high and pure quality, but also in the production of a sufficiently pure iron, which has hitherto, in this country, been the great bar to our success; and this is rendered all the more necessary as the quality of steel produced by cementation is very inferior in tenacity. Messrs. Cammell and Co. and Messrs. H. Bessemer and Co. lately competed as to who would produce the softest specimens of steel,—these specimens were of that quality used for boiler plates; and Professor Fairbairn states that no better proof of the perfection and reliability of the Bessemer process can be adduced as practised in England, than the close correspondence of the results obtained with these samples. These two different establishments have no connection with each other, and are entirely independent in their practical management. They both make a certain quality of steel to be used for boiler plates, and the results shown by their products under the test are so nearly alike, both in tensile and compressive strength, as to fall almost within the limits of observation.

Notwithstanding the high quality of Bessemer steel for all practical purposes, certain of the provincial makers have found difficulties in the manipulation of large masses, and state that, under the hammer, and when even made from the purest material, the result as to solidity is somewhat uncertain; and in consequence a variety of patents for new processes have been taken out for its further improvement.

Mr. Kohn gives a very interesting description of a process which he describes as being one of the most scientific methods of producing steel castings—namely, that invented by M. Pierre Martin, of Sireuil, in France:—"M. Martin melts pig-iron of a good quality, such as is used in the Bessemer process, in a Siemens or gas furnace: he works at a very high temperature, and by the action of a highly oxidizing flame upon the surface of the iron, succeeds in removing the silicon and other impurities from the iron, leaving only an excess of carbon in the liquid mass. This is then tapped from the furnace and cast: the castings, after being sufficiently cooled, are placed in another Siemens furnace, having a lower temperature, and a flame slightly overcharged with gas. This flame effects the process of cementation in a very perfect and uniform manner, the carbon of the iron being taken up by a part of the carbonic acid contained in the flame. As the process continues, the temperature of the furnace is raised, the flame being always contained

in its neutral character, to prevent the oxidation of the surface of the metal by the action of free oxygen. After a certain time, which depends on the thickness of the article to be operated on, the process of cementation is complete, and the casting is converted into steel. M. Martin has exhibited some very fine castings of that kind at Paris, and his process attracts a great deal of attention on the part of steel-makers. M. Emile and Pierre Martin have also introduced another steel process, which is now making rapid progress in France under the name of the Martin process. This consists in melting pig-iron in the Siemens furnace as described before, and in adding to the molten mass a suitable proportion of wrought iron, steel, or pure iron ore—a process patented and described long before by several inventors, but never as yet successfully and practically carried out. The Martin process, as far as it is known at present, is a practical success. It has recently been introduced into several of the largest establishments in France, and is working to satisfaction everywhere. The principal advantage of this process consists in the facility which it affords for using up old iron, and converting it into steel."

Amongst the many patents which have of late attracted the attention of the metallurgic world may be mentioned one or two very peculiar instances which owe their origin to Bessemer's idea. Two or three Glasgow men have devoted themselves to this object, and patented their plans.

Mr. W. Hadin Richardson, of Glasgow, thus describes his patent. "This invention has essentially for its object the diminution of the time occupied for bringing a charge of iron to nature in the puddling furnace, as well as for producing an improved quality of metal."

In lieu of the ordinary rabble or paddle at present employed by the puddler for turning over and working up the charge of metal, or in combination therewith, a tubular rabble is used, through which atmospheric or other air, gas, steam, water, or other fluid or solid matter in a pulverulent form, is introduced under, into, or upon the charge of molten metal in the puddling chamber, for the purpose of facilitating the process. The tubular rabble to be employed may be made of iron, or partly of iron and partly of fire-clay, or of iron and platina, or platina alone, or any other fire-resisting material. The patentee claims that by his process he gets an improved quality of metal, which is exceedingly tough, fibrous, and pure; and in contradistinction to what is known as Bessemer metal, it is capable of being as thoroughly welded as the best brands of commercial iron. It may not be necessary at this time to do more than bring this up as one of the many ingenious and scientific ideas resulting from this

new era in metallurgy, for as yet I believe it has had no practical result.

Another process has been lately brought forward, which, although not yet patented, will doubtless be so shortly. The exact nature of the invention is not yet published, or at least its exact chemical bearings, but the rationale may be so far explained.

"The process commences with pig-iron, which is melted in an air furnace in the cupola or in a crucible. After fusion, a certain percentage of the chemicals employed by him is added to the liquid mass, and the result is an almost instantaneous separation of the carbon from the pig iron, the new addition taking the place of the carbon in the iron, and the carbon appearing at the surface of the liquid metal in the form of graphite, which can be skimmed off and removed if the surface of the metal is kept sufficiently quiet. Further, the silicon is thrown out by another chemical addition. The silicum comes to the surface combined with one atom of oxygen as oxide of silicum—a white, dry, incombustible powder, which is insoluble in water and in sulphuric or nitric acid. By this mode of treatment the carbon is removed from the iron before the silicum, while in all the processes which refine the pig iron by oxidation the silicum is the first to be driven off. The substances added chemically take the place of those removed; and although the result is not what may be understood under the present definition of steel, yet the new product is a kind of steel, in which the constituent carbon is replaced by another material. The character of this new steel is that of peculiar hardness, although this quality can be regulated. It is stated to be superior in respect of temper to the hardest kinds of steel, and is therefore peculiarly suited to the manufacture of cutting tools. A number of tools manufactured from this peculiar material can now be seen in operation at the Anderston Foundry Company, and the results are very remarkable. The speed at which cast iron is turned at the lathe is from 30 to 36 feet per minute, and soft iron is turned at the rate of 50 to 60 feet. The tools made of this material can be worked at this rate for a whole day without requiring to be sharpened."

These statements are very remarkable, and, if true, will make a vast change in the manufacture of iron and steel, so far as cutting instruments are concerned, and reduce it to its simplest expressions. The practical effect of such a simplification cannot be over-estimated, or its effect exaggerated in the commercial value of steel; and if absolutely and practically true, Bessemer must look to his laurels.

Another Glasgow gentleman, Mr. Henderson, who has made valuable discoveries in the manufacture of ferro-manganese, in connection with

his operations in the reduction of copper from the residuum or waste derivable from the manufacture of bleaching powder, has been conducting experiments on a large scale for the purpose of making steel direct, and uses for this purpose the oxide of iron, purified from all admixtures, and in a state of mechanical aggregation which renders it very suitable for speedy reduction.

The precise nature of Mr. Henderson's experiments has not yet emerged, as he is at this moment engaged in securing his new process by patent; but sufficient has transpired to show that, if successful, a great change may be looked for; for, working as he does with the present materials, his new steel will in all probability be of a very high standard; and having unlimited supply of the oxide, the price may be expected to be moderate. The importance of this subject may perhaps plead my excuse for so long dwelling on these different principles; but day by day we have evidence of being on the eve of great discoveries; and although I might tell you of what Mr. Mushat, Mr. Parry, Mr. Chubbe, of Brooklyn, and many others, have brought forward on this subject during the past year, sufficient is before us to rouse our interest, and make us wish God speed to one and all.

II.—*On some Points in Certain Theories concerning the Purpose and Primal Condition of the Great Pyramid of Jeezeh.* By Mr. St. John Vincent Day, C. E.

Read January 20, 1868.

The subject of this paper is one concerning which the widest varieties of opinion have existed from the earliest times down to our own. The solution of that time-honoured question, seeking an exposition upon an immovable basis, as to the purpose for which the Great Pyramid was built, has occupied some of the most studious hours of many notable men in all those countries wherein education has advanced, and of many grades of society—men engaged in various spheres of action which belong to a high state of civilization; and yet, at the present day, it is difficult, if not in many points impossible, on account of the great diversity of creeds, and the evidence upon which such are held, to say that what is *popularly*, in contradistinction to what is *scientifically* considered as proof, can be found to bear out any one of the opinions that have been advanced.

As a starting-point, it is necessary for us to bear in mind, that the

solution of all questions bearing on the purpose of the Pyramid builders in creating that structure is mainly to be effected by processes of investigation, based upon the sciences of geometry, arithmetic, and mathematics, pure and applied, as well as upon several branches of physical and archæological research; whilst certain other of its features have recently, in a most marvellous and unexpected way, been shown to interpret several hitherto unexplained or wrongly explained passages of Scripture. On the latter it is not my intention, nor have we time, to touch to-night. Let us, then, approach the subject carefully, unbiassed, accepting nothing but what is proved actually or by remarkable convincing coincidence; and as the interpretation of the Great Pyramid depends in so many respects upon numerical data, let us also bear in mind that a most exalted principle of modern science is, as it has been lately well expressed, "not to propound perfect and infallible dicta, but rather to know all about the limits of error attending every numerical datum;" and having ascertained their exact values, we are in a position to draw our conclusions, but not before.

I am fully conscious of the snares surrounding that question of questions which I dare venture to approach; neither do I come before you this evening without being aware of the pains and penalties that others have sought to inflict, by the lash of a half-taught opposition, on those who, possessing some inkling towards it, have earnestly sought to explain the meaning of the primeval Pyramid. In all probability there is no other question which man has ever found himself compelled to ask himself, that is circumscribed with so many points suggestive of answers, often eminently satisfactory for a long length of their course, but which by and by lead to others, or a series, wherein another or more than one question again arises, which by its very nature and occurrence detracts from or disproves the probability of a correct inference having been originally drawn.

Literary men, as contradistinguished to what we moderns understand by scientific men, as well as mathematicians, historians as well as travellers, professors of various creeds of religion as well as statesmen, physicists as well as archæologists, philologists, Egyptologists, and hierologists, have each in their turn endeavoured, by means of the various special modes of treatment brought to bear, to solve the most ancient material mystery that our world has ever been called upon to witness; and what stands out as so characteristic and significant a feature of this grand old Pyramid, is, that it presents so many points calling for special investigation, one growing out of another, thus probably containing a richer store for seeking after than any other problem which our race has ever encountered. Then, although it has

happened that the nature of its purpose has through long ages stood so mysteriously silent, is that any reason, it may well be asked, why labour towards finding out its portent should be ceased? On the contrary, we are, I think, most solemnly involved in lending all the aid we possess towards discovering the intention—the purpose—of that gigantic and mightily majestic mass of masons' work which, during the lapse and turmoil of 4,000 years, has looked upon the stern vicissitudes of Egypt's troubled land; but which for ages too, exhibiting itself, though rudely, by the robbery and devastation of ignorant and ruthless leaders, still stands supreme, the most ancient and perfect, while in character most exalted; it is yet the noblest building on the face of the earth, securely sitting in tranquil ruin upon its desert hill, overlooking, as if bidding stately defiance, and showing itself yet unconquered.

The subject is, then, one which I approach with much diffidence; and I can only wish it had fallen to the lot of abler treatment than mine to deal with. I desire, therefore, to explain that my reason for taking it up in the way I do before this Society is partly because it is widely felt the time has now come when studious men must harness themselves with a deeper inquiry into the subject than ever; and I should not even yet have attempted this task but for the further reason that what is generally considered to be a very one-sided and weak, though perhaps *popularly* not *scientifically*-felt assault upon a very exalted and highly probable theory of the Great Pyramid, has recently been brought before the Royal Society of Edinburgh by Sir James Y. Simpson, who, I regret to assert, appears to have adopted the most unreasonable and superficial mode of attempt to upset what bears so large a shadow of truth, that can well be conceived. Sir James, in the early part of his attack, has, I may be excused for so saying, committed himself to the gross popular error of classifying the Great Pyramid with all other so-called pyramidal structures in Egypt, as well as in Ireland, forsooth! but then we should like to know what Sir James thinks of these far more marvellous than the Irish mounds—these entire ranges of so-called pyramids which are crowded together over vast regions of the American continent, the mounds of the Scioto Valley, those skirting the Missouri, Mississippi, and Ohio, or more particularly those of various parts of Mexico, and that vast pile, the great mound of Miamisburg—some of which are characteristic of, and some strikingly congruous with, the mounds in Ireland, as well as many another place in our world, and which are shown to have been for four purposes—namely, sepulchral, defensive against the inroads of enemies, and sites for the temples and sacrificial altars dedicated to those gods whom the erectors worshipped. Many of the works on the American

continent are, to some extent, pyramidal—that is to say, they stand upon rectangular bases, and their four sides slope at the same angle; but I do not know of a case on record of any one of them terminating in a sharp pointed apex; whereas, of the alleged Irish pyramids, that of Newgrange in particular has been shown, by the most conclusive evidence, never to have been pyramidal at all—a mere roughly-shaped truncated conic mound, with inclined sides connecting it to a flat top.* Now, to any one who will take the trouble to compare the Great Pyramid with any other masonry structure, the features at once show themselves so distinct that no doubt can remain but the Great Pyramid is perfectly unique, and stands out alone, unapproached by any other building, either externally or internally, that has ever been known to exist.

But of the various points and theories that naturally suggest themselves to our consideration, we must content ourselves this evening with examining one or two only, for the number is so great that many evenings would be necessary to submit the Pyramid to an exhaustive examination. Of the numerous theories that have from time to time been promulgated for us to give assent to, there are but two that need be considered—these are, the "tombic" theory and the "metrical" one. The others, from their very absurd and impossible nature, it would be folly to touch upon.

The Pyramid's Primal Condition Externally.

The foregoing remarks bring us at once to the question bearing on the primal condition of the Great Pyramid, as well as so-called (by some persons) pyramidal structures in general; for Sir James, in one part of his criticism, attempted—nevertheless, in howsoever powerless a manner—to indicate that "in various parts of the world very large sepulchral conical hills still existed, made sometimes of earth and sometimes of rubble, containing within them relatively small chambers for the dead, constructed of enormous stones, and galleries leading to these chambers." Now, before we either dissent from or assent to his

* From the very character of this Newgrange structure—a mere heap of loose stones—we may feel perfectly certain that it never could have been anything else than a very roughly shaped truncated conical mound. With such material as that of which it is composed, there are abundance of reasons for showing that it never could have terminated in an apex with its present side angle, and no doubt originally the angle approached the vertical more nearly than at present. The structure, probably, was made as high and steep as the angle of repose of the material would permit. For a further exposition on this point, see Appendix.

views, it behoves us to inquire into the original condition of some really pyramidal buildings. Then, first, as to the primal external condition of such structures, let us consider that of the Great Pyramid, gathering what we can from the writings of that old Halicarnassian—the usually faithful, and always considered reliable narrator of what he himself saw and was told—Herodotus; as well as from the order of things that the Pyramid itself presents to us at the present day. What, then, does our historian, in whose day the Pyramid was entire, say of it? He distinctly describes it as being then completely encased with a covering of hard, white limestone (which, by the way, has since his day by some persons been erroneously called marble); that each of its sides constituted a beautiful, smooth, and steep plane, so that the summit was inaccessible. But Herodotus has handed down to us something of far greater import than this. He, who lived in an age when the people of Egypt had fallen into a despicable condition of idolatry and departure from the greatest of great worships, that of Truth, and who had, therefore, in all probability been deprived of the whole true understanding of the Pyramid, but amongst whom a handing down of something pertaining to quite another meaning than *tombic* still existed—he indeed points out that a *metrical* meaning was understood to be involved therein in his day. No matter how imperfect and wide of the truth that which Herodotus heard is, still it shows clearly that in earlier ages, when Egypt followed intellectual and obedient, in place of idolatrous and revolting leadership, a higher and, in all probability, a complete understanding of its purpose was known; and we may, therefore, with great probability—amounting, indeed, as much as anything can, at this distant period, to certainty—judge that what Herodotus gathered was merely the fragmentary end of a once whole insight, which had become degenerated and lost through the failure of that people to follow true guidance. Then what did Herodotus learn in proof of an anciently understood metrical meaning being involved in the structure of the Great Pyramid?—namely, this, "that the area of one of its sides was equal to the square of the height." Such was written by him more than 2,000 years ago; and it is of grave and most weighty importance, as indicating to modern nations that the notion of a metrical meaning being involved with that ancient structure is by no means of recent birth; for although this belief has only of late years gained a fair share of attention from men of science, we should nevertheless remember that in Eastern countries there has always existed a tradition that, amongst other things, such as treasure or something connected with religion, science still was therein embodied, in contradistinction to certain other wild theories of Arabian authors and followers

of the false Prophet Mahomet, applied to even the Great Pyramid and its somewhat kindred structures adjoining it at Jeezeh—those of Dashoor, Sakkarah, and elsewhere.

But let us not trust implicitly in Herodotus by his own statements; rather let us seek, by the light of these later times, to test the veracity of his account. For about 1,000 years the Great Pyramid has presented no such exquisite surface as Herodotus describes, the surface being what has been described of a "stepped or laddered character"—rough, weathered, and morose-looking; but then for centuries past the Pyramid's base has been fortunately (and I use this word in a special sense) covered up and protected by the great mass of débris which had therearound, through long ages, accumulated, when, thanks to the labours of the French Academicians attached to the Napoleonic Expedition at the end of the last and the beginning of the present centuries, the sockets or holes of two of the stones forming the angles of the original casing were discovered and laid bare—but these corner stones themselves ruthlessly stolen away. A most memorable period in Pyramid literature is the month Pluviose, of the French Republican era (January, 1801), when MM. Le Père and Coutelle discovered the north-west and north-east corner sockets; and the sockets, although empty, nevertheless, their occurrence was enough to prove the former existence of the beautiful covering described by Herodotus; and as units providing for the concretion of the whole, their being found is most significant. Thirty years and more passed by, when at last Colonel Howard Vyse came on the scene of labour amongst the monuments of ancient Egypt, with what result so far as concerns the Pyramid? Why, this, that he succeeded in finding two of the lowermost course of casing stones, and part of a third *in situ*, unmoved, there-adhering, firmly cemented, to the old stepped or laddered inner mass from a day nearly 4,000 years old. The joints of these casing stones showed themselves to be of the most exquisite workmanship that even we highly structural moderns can well conceive; for, when measured by the Colonel's engineer, Mr. Perring, he found them to be 4 feet 3 inches thick on the upper horizontal plane, 8 feet 3 inches at the bottom, 4 feet 11 inches at the back, and 6 feet 3 inches on the slant side, or hypotenuse, the joint surfaces being so true at the edges that the intersecting cement at these parts was barely appreciable; although, from the well-recognized system of rigidly bedding large blocks of stone which the Pyramid builders so well understood, there is little doubt but that the parts of the joints nearer to the centre were hollow. Until, however, these were discovered, no proof in a material form remained in these latter days that the exquisite stone casing brought from the distant

limestone hills of Mokattam ever existed, to confirm the account given by Herodotus. It had been handed down to us that between the years 800 and 1000 A. D. the casing was robbed by certain Khaliphs, for the purpose of furnishing the material out of which were built the aqueducts and mosques for their new capital of El Kahireh—the modern Cairo; also that two enormous bridges or raised platforms were constructed to convey these robbed casing stones across the alluvial plain to the banks of the Nile. Sir Gardner Wilkinson sets all this forth very clearly; and yet with such facts known in Egypt, the modern Arabs, or some at least of them, assert that the Great Pyramid always had the (so-called by them) stepped or laddered exterior.* Most fortunate, then, is it that two and part of a third of these casing stones have in our day been found, and now, it is to be hoped, in part exist, protected by the overlying mass of rubbish; † while all the greater proof shows itself in the numerous fragments of similar casing stones, all cut to one definite and unmistakeable angle, which have been picked up in large numbers by Professor Piazzi Smyth during his recent sojourn at the pyramids, and to the unmistakeable nature of which I am able to testify, through his kindness of allowing me to examine them.‡ Let disbelievers in the original angular value of the sides of the Pyramid examine the large casing

* It is most singular that modern men should, some of them, persist with this dogma, when so many writers (although they are never to be trusted as to the interior of the Pyramid, which, according to the various accounts, must have changed itself from age to age as each writer lived) speak of the taking down of the casing.

† At the time these casing stones were uncovered, during Colonel Howard Vyse's explorations, they were much broken and disfigured by the Arabs and visitors to the Pyramid; so that, to prevent their being carried quite out of existence as a whole, the Colonel wisely ordered them to be covered up again. The Colonel states, however, that they were uncovered after he left, and *broken in pieces*. Doubtless, at some to-be-hoped not-far-distant day, when the platform on which the Pyramid stands shall be cleared on all sides, more of these important relics will be found.

‡ In a letter dated February 22, 1868, Professor C. Piazzi Smyth states to me, "Every single block, of all the hundreds of blocks which I examined of the *granite* part of casing of the Second and Third Pyramids, were so puddingy, decayed, and boulder-become near the edges, that no *hand-specimen* could have been knocked off that would show it had once been a casing stone; while with the limestone casing of the Great Pyramid you may find fragments of half an inch, which will give the casing-stone angle of two meeting planes." Such facts speak for themselves, and are strikingly significant that the Great Pyramid was intended by its architect (and he endowed with extraordinary foresight and understanding) to have been handed down through ages in its original form, and without alteration in its most important angle of side.

stone fragments * fitted into the model vested in the hands of the Royal Society of Edinburgh, as exemplifying one feature at least of the modern metrical theory of the Great Pyramid. They will then be more inclined to believe from that, than mere writing or mathematics appear to convince them—that is to say, if such exception-takers be convincible at all about anything as definite as fallible humanity can produce it.

All the facts which I have now brought together prove that not a shadow of doubt remains as to the reliability of that part of the account of Herodotus which is descriptive of the original external condition of the structure.

The Present External State of the Great Pyramid.†

To make our comparison complete, we must consider the *present* external state of pyramidal structures, for even in this respect it is most necessary, in order to draw just inferences, to examine together the qualities of workmanship. The present exterior of the Great Pyramid now exhibits four stepped flanks, each so-called step forming a complete horizontal layer all around, and extending inwards for a considerable depth—nay, from the very character of the structure itself, there can hardly be a doubt but that each such layer extends completely throughout the mass, except, of course, in such places where the interior hollow of chambers and passages occur. The masonry of which this mass is constituted is of a very high order, the stones being all well squared, and built together so as to constitute a complete system of break-joint, each stone being thoroughly cemented to its

* What is thought by some others, at least in Edinburgh, besides Sir J. Y. Simpson, may be gathered from the label of the specimen case in the Museum of the Royal Society. It runs thus :—

Fragments of the ancient and long
lost "casing stones" of the Great Pyramid,

DATE 2170 B. C.;

Recovered A. D. 1865, and here compared practically with
models of the side angles of a theoretical π Pyramid,

(The π quality being believed to be peculiar to the Great Pyramid alone, of all the Egyptian, or indeed any other, known and measured Pyramidal buildings.)

See Transactions of Royal Society, Edinburgh, Vol. XXIV. p. 385.

† In all cases where the ancient exterior coating, of whatever material that may have been, no longer remains, it is obvious that "present exterior," as in the Great Pyramid, really means *original interior* condition as finished by the builders.

neighbour; whilst in the locality of the passages they are shaped to fit the accurate inner lining there introduced. And how clearly does the exposure of this inner and never-intended-to-be-weathered mass show the wisdom of the builders in selecting the material of the Mokattam hills for the original, and, by them devised, *perpetual*, exterior! For although the inner structure has been exposed to the effects of rain, sun, and wind for less even than 1,000 years, yet these atmospheric changes have so gnawed away and destroyed its primitive condition, that in many places great rents are exposed, and again under-surfaces laid bare; whereas the original casing of a material not sensibly harder, and which had been exposed on the Pyramid's sides to all the vicissitudes of climate extending over 3,000 years, to this day retains a surface nearly untouched—discoloured certainly, as a necessary chemical consequence resulting from the elements of which the substance is composed, but unaltered in the sharpness of angle and outline originally imparted.

The Second Pyramid of Jeezeh.

Let us then inquire as to the collateral features in the second pyramid of Jeezeh. This, the second in importance of pyramidal structures, is situated on the same hill of nummulitic limestone whereon the grander Pyramid sits, lying in a direction south-west of the latter. There can be no doubt about this having been covered partly in a similar manner to the Great Pyramid, as a very considerable portion of its topmost casing still hangs around, whilst that below is likewise robbed away. But this structure never had a complete casing of Mokattam stone, the two lowermost courses being of red granite, the huge blocks which formed it, when perfect, still remaining—some but slightly disturbed, others scattered about on the level plain which had been excavated to receive this second pyramid. "The Muslim spoilers of the years 800 and 900 A. D.," says Piazzi Smyth,* "took the limestone and left the granite—just, in fact, as their Muslim successors do now; for they advance not, neither do they change; and day after day did we see camels from neighbouring villages loading up limestone blocks at the tombs around the pyramid, and dragging them off, but studiously avoiding the intractable granite,"—proof of this former ruthless theft being further extant in the fragmentary bits of the casing stones interlying the rubbish-heaps accumulated on all sides of its base. In the exterior of the two pyramids, then, there formerly existed some similarity; but the very introduction of the unmeaning granite base-

* *Life and Work at the Great Pyramid*, vol. i., p. 207.

courses of the second, distinctly show it to have had no oneness of design nor that simple purity of grandeur which attaches to the first. This, then, we may consider to be a first point indicative of a difference of purpose belonging to the two structures. Besides, how distinctly does not the mere introduction of what is certainly the more expensive and harder material indicate the ignorance of the builders as to the durability of the two substances! There can be little doubt but that the work of the Great Pyramid builder was intended to be handed down through ages in as perfect a state as the permanence of mundane materials and workmanship would admit; and therefore we should naturally expect that any man or set of men who had such wish in view should at once set about finding out the most likely substance with which to achieve their purpose. How perfect the knowledge in this point of durability of material must have been either to themselves or their leader, whoever that may have been, is apparent to any judge who has had the opportunity of examining the present condition of what remains of the Great Pyramid casing stones. Here we see clearly a mighty intelligence governing the work of the first Pyramid, and a wretchedly ignorant half-copyism of that noble work portrayed in the second: the first finished with a surface homogeneous, equally enduring throughout; the second finished with a casing partly homogeneous, but the remainder as discordant and equally unenduring, as proved by the decay and wasting away of the edges of the granite blocks forming the two lower tiers of casing stones, which remain to this day; whereas what is left of the upper part of the casing of the second structure still holds good its original form, wearing down but very slowly, and that without appreciable unevenness throughout.

But greater proof than what we have yet given shows absolutely the entire dissimilarity of the two structures. We had occasion to consider the character of the internal structure—that is to say, the present exterior of the Great Pyramid. Let us now look at that of the second; and what do we find it to be, but a poor, disorderly, altogether inferior mass of masons' work? It is described by Colonel Howard Vyse as "being only a kind of gigantic rubble work, so irregularly built, that since the removal of the casing, the desert sand and rain have penetrated in several places to a considerable distance; and it is owing to this looseness of construction that Signor Belzoni was unable to work his way through the stones, which had collapsed in the forced entrance, supposed to have been made by the Khaliphs; and that in 1837 the Arabs could not be employed in another part of it." But it is worthy of remark that this inferior class of work extends to only a little above one-half in height of the whole structure, the upper part

to which the casing stones still adhere being of a higher order, equal (I gather from the accounts of Piazzi Smyth, who appears to be the only observer of this distinction) to the inner structure—that is, the present external surface of the Great Pyramid. It is most probable that he would not have discovered this more so than others, but for the recent falling off of a part of the interstitial coating of stonework, which exposed to view the inner structure. What greater architectural blunder, then, could have been committed than the placing of a thoroughly-tied-together breakjoint system of well-squared stone superstructure upon such an inferior basement? Thus far, then, I venture to believe I have completely proved, by the evidence of the present state of things at Jeezeh, that in the matter of present exterior there is no similarity between the great and second pyramids. Let us see how the comparison fares with the third.

Other Pyramidal Structures.

The third pyramid of Jeezeh is situated, again, to the south-west of the great and second, as well are the fourth, fifth, and sixth. There is good reason to believe it was built long after the second, and therefore after the first. It was partly cased in granite, only to a much greater extent than the second—we learn, for at least half its height—and to this finish it is supposed to have had ascribed to it the name of "coloured pyramid." It is considerably smaller than the two others which we have considered, being less than half the linear dimensions of the Great Pyramid. A great number of these granite casing blocks still remain in their original position, but are so much defaced and destroyed that they are of no value in enabling any exact determination of the original angle of side or exterior linear measures being obtained from them. We may pass over the smaller pyramids of Jeezeh, which are too insignificant to bear on the present inquiry at all, and have no reason to be compared with the Great Pyramid, as to the purpose of which this paper is mainly directed to explain. Continuing on about 12 miles southward of Cairo another group of pyramids—those of Sakkarah—make their appearance, being about 4 miles inwards from the west bank of the Nile; most conspicuous amongst which is that one which has long been called the "Pyramid of Steps," so named from its present exterior appearance. The original base length, according to Perring, on the north and south sides, was 4,219 inches, and 4,627 inches on the east and west, whilst the present vertical height is 2,258 inches. The sides of this pyramid, too, are very far from being oriented as the Great Pyramid's are, the north end of the east side being 4° 35′ east of true north. The present exterior of this pyramid shows

a rather fine class of masonry; but as to its having ever been cased, like the Jeezeh pyramids, I do not possess information, although I think it probable that it was. As to the other pyramid, of Sakkarah, it is only necessary for me to draw attention to the low order of masonry which enters into its construction, as shown in the photograph, to prove that it is no contemporary work, or even approaching the high character of the Great Pyramid.

It is not, however, expedient for me to take up the time of this Society by continuing to make comparisons of this sort. I might carry them to much greater length; but I trust that it will be taken for granted, since it can be most clearly shown, that not another pyramidal structure in the world in any degree approaches the proportions—and I do not mean by this, mere linear dimensions—but the proportions of the parts, *inter se*, that obtains in the Great Pyramid. Long confessed to be *almost* the oldest, Dr. Lepsius, the greatest living hieroglyphic authority, has seen reasons for considering the Great Pyramid to be no longer "almost" but *absolutely* the oldest of all the Egyptian pyramids, and to have been reared up long before history was born into the world. There are other structures in Egypt, such as those of Dashoor, Abou-Roash, Meroë, and in other lands, which resemble it in form somewhat, but not in symmetry, strength, or vastness. Not the tumuli of Asia Minor nor the Tauric Chersonese, as well as the so-called pyramids of America—not the tomb of Alyattes, nor the pyramid of Cholula, nor those in the Euphratean Valley,—and I will descend lower still to Sir James Y. Simpson's ridiculously-brought-up Irish tumuli, watch-towers, and defensive works,—are worthy of comparison.

The word "Pyramid," its Derivation and Meaning.

Sir James Y. Simpson calls into question the signification of the word pyramid, and endeavours to assert that its derivation is from two Coptic* words signifying the "king's sepulchre." We will, then, for

* At the time of writing my paper, that of Sir J. Y. Simpson's, read to the Royal Society of Edinburgh on Monday, January 20, 1868, had not been printed in full from its author's hands. All that I knew of it was what could be gathered from an unusually full report that appeared in the *Daily Review*, January 22. I have, since the reading of my paper, obtained that of Sir J. Y. Simpson's as printed in the Society's *Proceedings;* but do not find that I have occasion to retract one word of that which I had originally written; on the contrary, it is necessary to be even more precise in print than the time allowed for reading the paper admitted of.

In his opening paragraph (*Proc. Royal Soc., Ed.*, Vol. VI, No. 75, page 243) Sir J. Y. Simpson remarks thus:—"After considering the many proposed deriva-

a moment, go upon the supposition that this assumption of Sir James's, which he bases upon the authority of Baron Bunsen's arch-supporter, Dr. Birch, is correct; and supposing it is so, we are equally certain that the words themselves were obtained from the older Egyptians amongst whom we believe some of the meaning of the Great Pyramid was traditionally understood. But this does not at all indicate that the signification of the ancient word for pyramid had any reference to a sepulchre of any kind; and there certainly exist the gravest reasons, when we consider the passion of the Coptic race for a means, as they supposed, of preserving the souls of their rulers after death in the securest possible manner, that they should select as their sepulchral model an imitation of the strongest and most enduring, as well as gigantic structure, that then existed, or that they could devise. Then, what could they propose to themselves as more fitting for such purpose than an imitation, as they supposed (without, of course, understanding the real meaning or symmetry), of the Great Pyramid? Hence it appears highly probable that the inter-

tions of the word pyramid, it was pointed out that the origin of the name suggested by the distinguished Egyptologist, Mr. Birch, from two Coptic words, '*pouro*,' 'the king,' and '*emahu*,' or '*maha*,' 'tomb,' the two in combination signifying 'the king's tomb,' were *probably correct* [the italics are mine]. *Men*, in Coptic, signifies 'monument,' 'memorial;' and '*pouro-men*,' or 'king's monument,' may possibly also be the *original* form of the word."

"Probably correct" is this inference to Sir James. Let us see if there is not some probability of its being incorrect. One thing is certain, that the absolute root of the word is lost; hence the number of proposed roots for it. Now, it is most singular that neither of the very learned gentlemen should have sought further back for the root of that word now called "pyramid;" than a *Coptic* origin. And it remains yet for Sir James and Dr. Birch to explain what they really imply by "Coptic;" because by most persons it is supposed to refer to people of a vastly later day than those cotemporary with the Great Pyramid's erection. Modern Coptic may be altered from an older language, but is not the old language understood as Mizraitic from Menes downwards. Supposing, then, that the ordinary meaning of the word is implied, it is equally singular that Sir James admits (*Proc. Royal Soc., Ed.*, Vol. VI., page 245) the Great Pyramid to be the oldest building in the world, by his believing in the discovery of Colonel Howard Vyse, that some of "the surfaces of some of the stones were found painted over in red ochre, or paint, with rudish hieroglyphics—being quarry marks written on the stones 4,000 years ago; and hence, perhaps, forming the *oldest* preserved writing in the world. Among these *accidental hieroglyphics* [perhaps the author of this sentence will yet explain in what sense they were accidental, when, on his own statement, they were 4,000 years old] Mr. Birch discovered two royal ovals—viz., Shufu (the Cheops of Herodotus) and Nu-Shufu." Then if Sir J. Y. Simpson is content to inquire no further for the origin of the word than the date of incoming of what is generally understood to be the Coptic race,—for, be it remembered that, by assenting to date, he tacitly admits the Great Pyramid to have been built by the pre-Coptic or Mizraitic nation,—he must further allow

pretation of the original word signifying pyramid had to the ancient Egyptians—the pre-Coptic race—a far different meaning than a "royal sepulchre;" and that through the gross degeneration of that people, the true signification of the word, and the true meaning of the structure itself, were lost; so that the Great Pyramid being imitated by the Copts as a burial-place for a king, they, no longer understanding the meaning of the word, used it to signify the king's sepulchre. The next point that here suggests itself is, whether we have any means now extant for ascertaining the original signification of the original word for pyramid?

If the metrical purpose of the building shall yet be fully proved—and it must be remembered that scientific men are advanced to a great distance in this line of investigation—there are very striking reasons for applying originally a mathematical meaning to the word. The metrical theory shows, from strict and rigidly careful measurement, that the

that, previous to the Copts, the Pyramid was either a thing not understood, and therefore almost necessarily without a name, or if possessed of a name, that name itself signified the structure as a thing not understood; for, if of Coptic origin, the Copts themselves must have christened it "Pyramid." What a powerful lever against the entire "tombic" purpose of the Pyramid, as pronounced by Sir James, then underlies his entire argument! It is, however, well to pursue the other proofs against the entire tombic views that unavoidably herein grow up, and to state the fact, once for all, that long ages before the Copts were, the Pyramid was. In the day of its builder, to him at least its exalted purpose and symbolization may have been known—perhaps purposes (for the metrical theory does not object to a very large amount of sepulture having taken place within it). Still, "tombic" purposes do not explain its exquisite structure, its particular and accurately defined exterior and interior angles, its chambers resting on definite and unmistakeably numbered courses of the builded mass, nor a host of other portentous symbols; for nowhere else than in Lower Egypt is to be seen the pure, simple, and scientific form of a *pyramid* actual and proper. It tells a clear and mathematical story of itself, which never could have been read after the day when the nations fell into that degrading, graphic, and idolatrous religion, until now, when the western world, to some extent at least, has sought the fellowship of, and learned the lessons taught by seeking after, truth. Modern physical and geometric science has been so far developed as to enable some of its foremost masters to go forth and examine that mighty mass by the light which has in these latter days been thus afforded. The other so-called pyramids are uninterpretable by it: in them there is no shadow of science to be evolved. Truly "tombic" are they, full of frippery, and altogether the base insignia of some of the basest worldlings.

I may justly add here a most trite and fitting comment that has recently been made:—"Though we who do our duty as well as we can for the Pyramid may have our failings and make our mistakes, we are extraordinarily assisted by the huge blunders of such opponents."

original vertical height of the Pyramid was to the length of two sides of its base as the diameter of a circle is to its circumference—or as

$$1 : \pi.$$

Now, in this sense, the Pyramid was originally a symbolization in the form of that one pyramidally-shaped solid which alone could represent the figure of the earth. From this I have deduced that the ancient word for "pyramid" signified, as one of the features expressed by it, that geometrical solid in which said proportion of height to base, or the mathematical quantity π, or the ratio of 1 to π, was embodied; and in this sense no other modernly-called pyramidally-shaped solid would be a pyramid at all, unless its height and base were in that exact ratio; and therefore the word really referred to that only one material standard known to exist anywhere; where those proportions could at any time be measured, either for mathematical or any other purpose whatever. Now, it is indeed most singular that our modern mathematical quantity π is the first letter of two Greek words,

περιφέρεια and *πυραμις*:

and it has, I believe, been generally supposed to be used to represent the ratio of a circumference of a circle to its diameter, because it is the beginning of the Greek word for periphery; but we may be equally certain that some symbol for this must have been known long before the Greek π or *περιφέρεια* were invented, and which we know the Greeks got from the Egyptians, both of which were derived from the older Mizraitic language, and are to it a comparatively modern contrivance, and therefore merely represent to us under this metrical meaning that there was an older symbol and older word for pyramid used by the Great Pyramid architects, and fully understood by them in all its valuable mathematical meaning—both in a pure as well as practical sense.

It is in this restricted sense that I conceive the original meaning of the primal word for "pyramid;" although, since, in the ages of Egypt's degeneracy, when she began to show up her idolatrous religion in that strangely graphic manner, the pure word was applied to signify so vain a structure for a purpose equally vain, it has since then by some men been used as a name for various geometrical solids, tapering from a base to an apex, of every conceivable degree of angle.

Is the Pyramid "Tombic" or Scientific?

It behoves us now to test the view I have thrown out by another consideration, for Sir James Simpson will not allow anything scientific

to be bound up in the Great Pyramid, although he has strangely attempted to show that the people who were cotemporary with the period of its erection were quite well versed in "mining, architecture, and astronomy; and were so advanced, in various parts of the East, as to present no obstacle in the way of the erection of such *a noble mausoleum as the Great Pyramid undoubtedly is*." In support of his tombic view he quotes the tale of the Arabian historian Ebn Abd Al Hokm, who, amongst some of his writings that have been handed down to us, says that when, about a thousand years ago, Khaliph Al Mamoon, in quest of treasure, tunnelled into the centre of the northern face, and by and by got into the king's chamber, "he found towards the top of the chamber a hollow stone, in which there was a statue like a man, upon whom was a breastplate of gold set with jewels. Upon this breastplate was a sword of inestimable price, and at his head a carbuncle of the bigness of an egg, shining like the light of day, and upon whom were characters writ with a pen which no man understood —a description stating, down to the so-called 'statue,' or painted wrapper or cartonage, and the hieroglyphics upon the sere cloth, the arrangements now well known to belong to the higher class of Egyptian mummies." It would be interesting to know if Sir James, at the time of reading his paper, was aware of the fact that there are several Arabian writers, of the same period as Ebn Abd Al Hokm, who make the Great Pyramid a scientific structure; and show that when Al Mamoon broke into the king's chamber, he found nothing but the *empty* stone coffer; and who, for fear of the attack by his followers upon him after so fruitless an end, in quest, as it was thought, of treasure, directed his men "to sink a hole near the end of his excavation," wherein he hid a large sum of money, and which they at last came upon. But out of the many Arab historians who give as many different and discordant accounts, Sir James hits upon one who makes the pyramid "all-sepulchral;" but he did not tell us that there are many such Arab writers, each with a different story—many of them much nearer to Al Mamoon's age than was Al Hokm; yet not one of them is of the least authority on the originals of the Great Pyramid.

It is therefore necessary, to enable us to draw a fair estimate of the value of the author to whom Sir James has so unconditionally confided himself, to hear a little of what other writers of his country and about his time, as well as before, do really say. Let us then first take the account of

ABOU MA'SHER JA'FER BEN-MOHAMMED BALKHI, who died 884 A.D., and writes, —"The wise men previous to the flood, foreseeing an impending judgment from

heaven, which would destroy every created being, built upon the tops of the mountains and in Upper Egypt many pyramids of stone, in order to have some refuge against the approaching calamity. Two of these buildings exceeded the rest in height. Upon the exterior of the building every charm and wonder of physic was inscribed in the Mosannad character, and likewise this declaration:—'I have built them, and whoever considers himself powerful, may try to destroy them; let him, however, reflect that to destroy is easier than to build.'"

MASOUDI, 967 A.D., says that Surid Ben-Shaluk Ben-Sermuni Ben-Sermidun Ben Sedresun Ben-Sal, one of the kings of Egypt before the flood, built the two Great Pyramids (the Great Pyramid and second pyramid of Jeezeh). In the eastern Pyramid (or Great Pyramid) were inscribed the heavenly spheres, and figures representing the stars and planets in the form in which they were worshipped.

The king also deposited the instruments and the thuribula with which his forefathers had sacrificed to the stars, and also their writings, likewise the positions of the stars, and their circles, together with the history and chronicles of time past, of that which is to come, and of every future event which would take place in Egypt. He placed there also coloured basins (for lustration and sacrificial purposes), with pure water and other matters.*

Says that Khaleef Haroun Raschid [others say, Al Mamoon] made a chasm in the Great Pyramid by means of fire, vinegar, iron instruments, and battering-engines; and having penetrated 20 cubits, he found a vessel filled with a thousand coins of the finest gold, each a dinar in weight; and the whole amounting to *exactly* the expense he had been at in excavating, he wondered thereat exceedingly.

ABOU ABD ALLAH MOHAMMED BEN ABDURAKIM ALKASI, died about 1187 A.D., says that "Al Mamoon opened the Great Pyramid, (820 A.D.)

"The author entered it and found a square chamber with a covered roof, and in it a well 10 cubits deep; whence at each angle doors opened into large apartments in which bodies had been deposited enveloped with many wrappers that had become black through length of time.

"He was informed that those who went there with Khaliph Al Mamoon came to a small passage containing the image of a man in green stone, and that when it was opened a human body was discovered in golden armour, decorated with precious stones: in his hand was a sword of inestimable value, and above his head a ruby the size of an egg, which shone like fire."

ABD ALLATIF, 1200 A.D., says,—"Many of my companions entered the opening into the Great Pyramid made by Khaliph Al Mamoon, and went up into the chamber constructed in the top of the Pyramid. Upon their return, they related the wonderful things they had seen, and that in the upper part were openings and windows which appear to have been made to admit air and light. When I again visited the pyramids I entered the passage with several people, but having penetrated about two-thirds into the interior, and having through fear completely lost my senses, I returned half dead.

"These pyramids are built of great stones about 10 to 20 cubits in length,

* In a footnote (*Pyramids of Gizeh*, Colonel Howard Vyse, vol. ii., page 326), Dr. Sprenger, who had translated the Arabian account, makes the following significant comment, "Every writer, indeed, seems to have enumerated as many marvellous things as his imagination could suggest."

and covered with ancient inscriptions in a language now unintelligible, as many inscriptions as would occupy 10,000 pages."

JEMAL ED DIN MOHAMMED AL WATWATI AL KANINI AL WATWATI, 1340 A.D., states that "to the Great Pyramid there are seven entrances, each composed of one stone, and closed by one lock with such exactness that they cannot be forced open; that there are seven chambers dedicated to the seven planets, and in every chamber an idol of gold, with one hand upon the mouth, and with an inscription in Sind upon the forehead; that upon reading the inscription the idol opens its mouth and discloses a key which admits to various apartments."

EBN ABD AL HOKM, about 1438 A.D., repeats much of what Abou Abd Allah Mohammed Ben Abdurakim Alkasi said in 1187 A.D. about what he was told Al Mamoon did in 820 A.D. Considers that the pyramids were built by Sheddad Ben-Ad before the flood.

SHEHAB EDDIN AHMED BEN YAHYA, 1371 A.D., states,—"The Great Pyramid was opened by Al Mamoon, but nothing was discovered as to the motive or time of its construction. Each of the pyramids was consecrated to a star.

"The Sabæans performed regular pilgrimages to the great one, and also visited the others, which were less perfect.

"Every pyramid presents the form of a lantern. It begins at its base as an equilateral triangle, which diminishes as it rises, so that it is surrounded by equilateral sides. This form indicates that it was sacred to a star."

The foregoing are a few samples, out of a great many more which are too numerous to cite, of the Arab authorities for the Pyramid; and out of whom Sir J. Y. Simpson has, for what reason we fail to see, selected and placed positive dependence upon, that one man in particular, who lived no less than 618 years after Al Mamoon forced his way. It is really idle to comment further upon the unfairness, nay, the absurdity, of the unlimited license which has been taken in making such a selection.*

* Whether Sir J. Y. Simpson had heard of my comments placed before the Glasgow Philosophical Society, concerning his choice of an authority (and which took place before the issue of that Number of the Royal Society's *Proceedings* which contained his paper) I know not, but there is reason to believe that he felt subsequent misgivings on this point, for at page 253 he endeavours to increase his stability by saying, in a footnote, that "It may be remarked," observes Colonel Vyse, "that the Arabian authors have given the same accounts of the pyramids, with little or no variation, for above a thousand years" (*Pyramids of Jeezeh*, Vol. II., page 328). Now, I beg to declare that Colonel Howard Vyse has never used any such words, either in the page quoted or anywhere else in the whole of his three volumes that I have been able to discover. The sentiment there expressed is, indeed, anything but his; for at page 179, Vol. II., he says, "The extreme antiquity of these remarkable monuments is evident from the uncertainty that attends their origin, and also from the *fabulous accounts given of them by Eastern writers, in which ignorance and superstition have so completely disguised tradition and facts, that it is scarcely*

SIR GARDNER WILKINSON, in Murray's *Handbook to Egypt*, says,—"The authority of Arab writers is not always to be trusted; and it may be doubted whether the body of the king was really deposited in the sarcophagus (of the Great Pyramid)."

Happily, too, the metrical theory depends, as it was lately well expressed to me, "on no follower of a false prophet, or upon anything written by such deluded men 3,000 years and more after the Pyramid was built."

But suppose we give full weight to the tale on which Sir James builds his attack, and admit that the Pyramid was used as a tomb, and it does seem extremely probable that those builders of the later tombic pyramids, the second, third, and so on, as well as those of Dashoor, Sakkarah, and elsewhere along the Egyptian valley, knew the manner of opening and closing of, and therefore the entrance into, the Great Pyramid—if they did not, it is difficult, seeing that they attempted to copy it in some other to them appropriate respects, in their tombic pyramids, to show for what reason they adopted a similar means of entrance into, and similar means of closing the passage leading to, their lower or sepulchral chambers. There can hardly exist any doubt but that the builders of some at least of the

possible to ascertain the foundations upon which they rest;" and he sums up at page 366, Vol. II., thus,—"*Indeed, the only fact which seems to be established by the Eastern authors is the opening of the Great Pyramid by Al Mamoon, and even of that no distinct or rational account exists.*"

The quotation which Sir J. Y. Simpson has given is from a footnote to Masoudi, and with Dr. Sprenger's name attached to it. Masoudi lived about 130 years after Al Mamoon; yet Sir James uses it without even mentioning his name in connection with the Pyramid, or even mentioning the account of any other person, of finding a body like that described by Ebn Abd Al Hokm. Masoudi, indeed, gives one of the fullest accounts of any as to the scientific, astrological, medicinal, and cabalistic (with a very small proportionate amount of burying) nature of the pyramids and their fittings, and it is to that Dr. Sprenger is alluding, as what the Arabs have uniformly stated for 1,000 years; what they received from the Copts, and they again from the earlier Egyptians. It would have been as well had Sir James quoted the whole sentence. Indeed, it is notoriously clear that the foregoing is the view Dr. Sprenger is considering, from his footnote at page 326, *Colonel Vyse, Vol. II.*, and quoted above at page 18.

At a meeting of the Royal Society of Edinburgh, held April 21, an occasion when Professor Smyth alluded to the unwarrantable nature of Sir James Simpson's attack, the latter read an extract from a letter of Dr. Birch, purporting to give *his* opinion that Al Hokm died within thirty years of Al Mamoon. It is only right that Sir James should let the public generally know what Dr. Birch really did write, also *when* he wrote it. Even then it does not clear Sir James, because he quotes Al Hokm from Vyse and Sprenger, who certainly believed him to have been 600 years after Al Mamoon, and this report of a private letter is not of itself sufficient to overthrow the universally admitted authority of Vyse and Sprenger.

later pyramids were fully well aware of how to get into the first. We know, at least, the probabilities are very great that the pyramids had been opened before the time of the Arabs; and from Sir Gardner Wilkinson—a most safe authority—in his *Handbook to Egypt,* we learn, as further tending to confirm such belief, "that the Egyptians themselves had, in many instances, plundered the tombs of Thebes, and closed them up again."

In the large folio usually termed *Perring's Plates of the Pyramids,* but got up and annotated really by Colonel Howard Vyse, the latter has clearly shown that he believes the pyramids were entered forcibly, but by the regular entrance passages, by the Egyptians, ages before the Arabs.

At page 2 of the second part of the above-mentioned work, it is remarked,—

"By whom the pyramids were first broken into will probably never be revealed; but as they were apparently entered by the regular passages, their interior construction must have been at that time known. It appears, likewise, that the Mahometan Caliphs, who, according to Arabian historians, made the forced passages, must have possessed, either by tradition or otherwise, some information respecting them."

It will no doubt be universally admitted that, in regard to facts concerning Egyptian pyramids which are of that nature, the more generally observed and sought after, and the deductions capable of being inferred therefrom, there never lived a man whose opinions are entitled to more respect than those of Colonel Howard Vyse. Unfortunate is it that he should not have been until the present time spared amongst us; for we may be sure that none more so than such an earnest investigator as he would have readily brought valuable aid in eking out the whole that yet underlies his own magnificent labours.

The breaking-in of the Egyptians mentioned in the sentence above-quoted was probably about 600 years B.C.,—and, let it not be forgotten, by Egyptians of a totally different faith to those of the Great Pyramid day; and such having happened unquestionably destroys any significance that may, on a *prima facie* view, be attachable to Al Hokm's report of Al Mamoon finding a body in the coffer of the King's chamber, supposing for the moment that he even did find it.*

Thus there indeed appears to be much strong evidence that these later pyramid builders, or some other people, operated in a very

* The reader should refer back to the quotations from Arab authors, and my comments thereon, at pages 18, 19, 20, and 21; also to the significant words of Sir Gardner Wilkinson, quoted at page 21.

sepulchral way in the Great Pyramid; for we find the entrance-passage of the Great Pyramid continued beyond the point where it unites with the lower mouth of the well, and so far as we can learn, at the same angle as the upper part, but abruptly terminating in a very large, roughly hewn, and unfinished chamber, generally known as the "subterranean chamber." Now, it has occurred to me that the existence of this unfinished chamber is of itself evidence, by its very character, of not being the work of the builders of the Great Pyramid (*one of whose chief distinctions, everywhere in that structure so plainly marked, is the exquisite finish of their work, which obtains in no other known pyramidal building*), but that of a later, and inferior, and ruder race of men, who knew nothing of the high character or peculiar upper chambers of the Pyramid, but who pulled out the closing stones of the entrance-passage, and finding it abruptly ending at the point where it meets the well, as the hole is called through which the architects escaped, after lowering the portcullis and the closing stones of the ascending passage; being confounded, ignorant of, and unable to conceive at the real meaning of this order of things, it occurred to them that the Pyramid would make, according to their base and despicable creed, a splendid royal tomb—so they continued the excavation in the line of the entrance-passage, until, coming under the centre of the building, they there commenced enlarging it into a chamber for royal burial. But the very fact that this chamber exists in such an unfinished state shows that these people were, by some cause, stopped short in their work,—very probably by finding out the bottom of the well, and by ascending which they reached the now-called King's chamber. This does, I think, appear extremely probable, and that these depredators, thus finding their way into the upper magnificent chamber, and seeing only the lidless stone coffer in it, thought to themselves, "Here is a fit sarcophagus and burial chamber indeed!" All this, no doubt, happened after the building of royal sepulchral pyramids had been begun in Egypt, for there is a striking analogy existing between the subterranean chamber of the Great Pyramid, and the subterranean, that is to say, tombic, chambers, which are the only chambers belonging to all the other pyramids which have been examined. There is part of an unfinished passage leading away from this subterranean chamber of the Great Pyramid, and we find similar passages leading to secondary chambers in those of the later pyramids, such as the second, as well as in the pyramid of Steps at Sakkarah.

The Coffer.

To advance beyond the mere speculation of probabilities, there exist

far more cogent reasons for vindicating the correctness of the view which I have attempted to throw out—namely, the facts concerning the dimensions of the coffer in the King's chamber of the Great Pyramid; which some men have tried to show is merely a sarcophagus, and lastly, Sir James Y. Simpson declares it—although without showing one scrap of real evidence in favour of his view—no other than "an old, and somewhat misshapen, stone coffin."

We will now examine our facts, and see if they do in the least bear out Sir James's dogma. In the first place, then, this coffer is considerably larger inside than any other known Egyptian sarcophagus; its proportions are quite unique, the mean capacity of its hollow, as shown by four different modes of determining it, being 71,250 inches, whilst that of the sarcophagus in the second pyramid is only 66,410, or, according to Howard Vyse's numbers, 64,554; that of the third pyramid being still less, namely, 46,219; that of a large tomb near the Great Pyramid being 51,117; that of the fourth pyramid being 29,610; and the fifth, 39,775 cubic inches; and others are still smaller. The distribution of this superior capacity is still more peculiar in the coffer; for although it may have what might be termed a fair sarcophagus length and breadth, yet it has depth altogether unusual, extravagant, and useless for burying a man in,—a depth also which would prevent the decent and supposed usual arrangement of the sarcophagus being taken to its place of final rest with its lid cemented and fixed on, as with coffins in the present day being conveyed from the house of the deceased to his grave.

Quite recently only has it been discovered, or at least recorded as a notable fact, that the coffer in the King's chamber had one most necessary character of an Egyptian sarcophagus belonging to the pyramid-building periods; and even this feature, which at first sight seems altogether for burial, and against a metrical theory, loses its force upon a closer and accurate examination. Before we can proceed to test this division of our inquiry, it is important for us to understand the manner of fixing the lids of Egyptian sarcophagi which belong to the Memphitic period. Thus,—one of the longitudinal top edges is quite cut away to a depth of from 1 to 2 inches, as shown in the drawings of sarcophagus of second pyramid, Plate I.; and at either end from this, two dovetail grooves are cut, with the top edge hanging over towards the centre of the sarcophagus, reaching from the lowered longitudinal edge to the opposite longitudinal edge, which is cut out in the same manner as the two sides. In this way a lid made to fit the grooves could be slid into place across the lowered longitudinal side, and when in place was quite firm, being

prevented from being drawn back by certain drop pins which were fixed into blind holes on the under surface of the lid, and when pushed home the pins fell down a short distance, entering corresponding holes of sufficient depth in the lowered side of the sarcophagus. The lid itself is always very thick and strong. The question then arises, Does such an arrangement present itself in the coffer of the King's chamber in the Great Pyramid? I answer certainly not; for there never has been found, neither has any one—not even Sir James Y. Simpson's (to him infallible) author, Ebn Abd Al Hokm—ever seen or heard of a lid belonging to the coffer. All writers have concurred in that one report, that the only thing contained in the chamber was the empty vessel, termed variously by them as a "hollow stone," "granite chest," "lidless box," "porphyry vase," and so on. But upon a little closer examination than appears to have been usual with other examiners, it has lately been found * that this coffer is lowered on its west side by the depth of 1·72 inches, and that from this depression ledges proceed of equal depth along the northern and southern ends, joined again by a ledge of equal depth at the east longitudinal edge. Now this, at a *prima facie* view, is a very sarcophagus-looking feature indeed, and, if examined no further, would, with some minds, completely upset the belief in the coffer being a metrical vessel. Yet of what nature are these ledges or grooves?—not dovetailed, be it remembered, as in the sarcophagus of the second pyramid and elsewhere, and by which shape, or some slight modification of it, alone could the lid be retained in place at all!! but these recesses or ledges are rectangular; that is to say, the base is a horizontal plane and the sides are vertical, as shown in the drawings of the coffer, Plate III., the grooves being marked *a*, *a*, at figs. 3 and 4; for although the coffer is miserably chipped away and disfigured (see drawings of its present state, Plate III.), still quite enough of these grooves remain to show that they never were dovetailed, but rectangular, and therefore not "retaining grooves" in any respect whatever, not being capable of affording the least security in the way of holding a lid. But some will ask, then, How came these grooves here, and the holes for the drop-pins along the lowered western edge? I have before shown my reasons, based upon the unfinished state of the subterranean chamber, as affording strong grounds for believing that such chamber is not the work of the builders of the Great Pyramid, but that of some after race in the days of Egypt's degeneracy, who, not knowing the real purpose of the

* By Piazzi Smyth, *Life and Work*, Vol. I., pages 84 to 90; also Vol. II., pages 115 to 117; and Vol. III., pages 147 to 150. A slight notice of the coffer's ledge, but very erroneous, is found in *Perring's Plates of the Pyramids*, above-mentioned.

Pyramid, set working to excavate a chamber for a royal sepulchre. Now, the very fact of the coffer presenting the appearance I have mentioned strongly tends to indicate that these men had found out an entrance by the well to the King's chamber; and seeing the then perfect stone coffer there, determined to use it as a royal sarcophagus, as proof of which, these *begun*, but *unfinished* and *improperly-shaped grooves*. Then, further examination shows that they were either forced to stop this converting the coffer to such use, or, what is more probable, they found impossibilities in the way of using it, on account of its mere size, and mainly its extravagant height, which would not have allowed them to take it through a doorway, or down a passage, with a lid on.

The mean of several measurements of the height of the coffer, and making all corrections for probable extremely small amounts of error, is 41·17 inches outside, the maximum being 41·3 inches; and the mean height of doorway to the King's chamber is only 42 inches. Now, in this case we find that the difference in mean height between the coffer and doorway is but 0·83 inches; this, added to the depression of the western side and ledge, would give a lid of only 2·55 inches thick, which is simply ridiculously out of proportion when compared with the mean thickness of the coffer's sides, of 5·99 inches, or within $\frac{1}{100}$th of 6 whole inches, and a thickness of bottom of 6·92 inches or nearly 7 inches. If we examine the dimensions of the sarcophagus of the second pyramid, we find the lid to be 8·2 inches thick to the part cut out, and 9·8 inches elsewhere, whilst the mean thickness of the longitudinal sides is 7·6 inches, and that of the ends 9·35 inches, showing the lid thus to be much thicker—in fact, the heaviest part of the sarcophagus. Can there be any reason assigned—supposing the coffer for the moment a mere "coffin"—why such a weak and easily-broken-into lid should be used in the strongest, grandest, and noblest Pyramid too? Besides, mere heaviness of this very part was one of the chief elements of its security. To have used the coffer as a sarcophagus, we must allow it a lid at least as thick as that in the second pyramid, in which case its height with the lid in place becomes 49·37 inches, or indeed 7·37 inches, that is, close upon 8 whole inches, higher than the doorway entrance to the King's chamber. This, in a very simple way, shows that the coffer—no matter for what purpose intended, as a capacity measure, or some other thing—could never have been used as a good sarcophagus, according to the believed ordinary mode of fastening on the cover before entering the royal tomb. Then some modern architect or clever worker in stone will say, Surely those who attempted to convert the coffer for burial purposes could as easily have cut away the under side

of the granite stone forming the roof of the doorway? This, at first sight, is a very feasible suggestion, and there are some indications that they thought the same, for we find at the entrance end of the doorway the granite roof stone cut away roughly for nearly 2 feet inwards, in the direction of the chamber, so that either it was found too difficult to cut away such stone, firmly fixed as it was, while the workmen had but such a confined space to work in,—and that, too, over a length of about 101 inches; or, what is more probable, that whilst this violation of the Pyramid was going on, some of those vast political changes between *North* and *South* populations and religions to which Egypt was subject took place, and so the spoiling was happily stopped; and we moderns—nevertheless the present ruinous condition—by the aid of modern science, have been enabled to revive the conception of what the Pyramid was, these spoilers not having been permitted to sweep the characters of the coffer and its original state quite out of the range of recognition. Again, there is another proof that the coffer is no pure sarcophagus, as the sarcophagi of the other pyramids and tombs are either sunk in the floor, with the upper surface of the lid a little above the floor surface, or, on the other hand, placed standing on the floor of a *subterranean* chamber; but here we see the coffer of the Great Pyramid never was sunk at all, but has always been standing upon a beautiful level floor of a most stately finished and *upraised* room, of truly cut and finely polished granite, moveable about on such floor, but to this day scarcely shifted, in all probability, from its first-placed site.

Against the question of the capability of removing the coffer with a lid on, out of or into the Pyramid, structural proofs are abundant that, in the case of the Second Pyramid, the sarcophagus found there could never have been used in the believed ordinary manner of burying a Pyramid King. I am for the moment supposing the sarcophagus in Belzoni's chamber to be that in which royal interment did, or was intended to, take place. The proofs which exist that it never could have been so used are its own dimensions, and those of the entrance-passage, which show that, with the lid on, that sarcophagus never could have passed up or down that passage, because the passage's mean breadth is 41·65 inches, whereas the mean breadth of the sarcophagus is 41·8 inches, or 0·15 inches broader than the passage itself. The mean height of the entrance-passage, perpendicular to its plane of inclination, is 47·3 inches; but the coffer with the lid on is 46·2 inches high. These dimensions assert that if Nu-Shufu or any other ever was buried in this sarcophagus, he never could have been, according to what is usually accepted as having been the pyramid

burial practice. Yet there is a way in which this sarcophagus would go down the passage—and the only way—namely, with the lid off, and turned over on its side, in which position there would be (47·3 — 41·8 =) 5·5 inches of space between the sarcophagus and the passage roof, and (41·7 — 38·0 =) 3·7 inches to spare between the sarcophagus and passage sides.

At first sight all this looks ugly, and disbelievers will exclaim, "Here is proof unmistakeable that the king's body was taken into the Pyramid in his shroud, his mummy exposed to the vile gaze of the enslaved multitude of subjects. Such being the case, why need we concern ourselves further about the question of capability of bringing the coffer out of or getting it into the Great Pyramid and King's chamber, when by this evidence it is certain that Cheops's body would and must have been similarly carried into the Great Pyramid?" Since opponents pretend to so much faith in historical accounts, let us see what history does say on this head of burial. And she asserts this, that in the case of the Second Pyramid, there is entire silence about any man's body ever having been buried therein, and there is no account of any ever having been found therein. History is equally silent, in any way that can in the least be trusted, about any man's body ever having been found, as placed by the original builders, in the Great Pyramid. Indeed, several of the earliest and most reliable writers state—amongst them Diodorus Siculus—that "it happened neither of the kings who constructed them was buried in them; for the multitude, enraged at the sufferings endured in the building them, and at the many cruel and violent acts of the kings, threatened to pull their bodies in pieces, and tear them insultingly from the tomb."

With regard to the Great Pyramid, Herodotus does tell us (he, too, of earlier date than Siculus) that Cheops's body lies deep down in a subterranean island, surrounded by Nile water (led thereto by a tunnel from the river); and there have recently been discovered certain special lines in the entrance-passage of the Pyramid which seem to point, amongst other things, to the spot where that tomb shall yet be found. Besides, what ground have we to think for a moment that, even if the enslaved populace did employ such a threat, they could carry it into effect?—as we know Nu-Shufu lived and reigned after his elder brother; so that, whether the two were co-regent or not, Colonel Vyse has acutely remarked, "If Cheops reigned fifty years, and had sufficient power to construct the Great Pyramid, it can scarcely be supposed that his body was not deposited within it, particularly as his successor is said to have reigned fifty years, and to have erected a similar tomb for himself, which he would scarcely have done had his predecessor's tomb

been violated, or any doubt existed of the security of his own." So that whether we trust the account of Siculus or that of Herodotus (and there can be no question about preferring either of them to Ebn Abd Al Hokm or any of his deluded countrymen), in either case we are equally sure, on the highest historical ground, that the coffer was not originally used as a sarcophagus for *Cheops.*

Discoveries in the Third Pyramid, however, have shed new light on the Great Pyramid; indeed, when rightly looked at, the more we examine it and others, so much the more do its exalted character and distinction grow up.

Turning then to the Third Pyramid: history does unmistakeably assert that a royal burial did therein take place,—and fragments of the very mummy case (an internal wooden box), with the very name declared by history found inscribed on it, as well as the regular burial sarcophagus (Plate IV.)—a grand affair of blue basalt, richly, deeply, and intricately carved in imitation of an Egyptian temple—in which said mummy was contained—yea, some say the very bones of Mycerinus* —have been found, and now lie in our own British Museum! But that sarcophagus is a very different thing from the coffer in the Great, and the stone box in the Second, Pyramid; a thing of dimensions more nearly proportioned to the size of a man; a thing capable of easily, and with its lid on and fixed fast, passing up or down the entrance-passage; alike in finish and other respects to some of the regular ornamental tomb sarcophagi; and placed, too, in a far-off, secret, low-down, closed-up, hidden, and little-to-be-suspected chamber—a chamber such as never has been found in the Second or Great Pyramid. And all the while this Third Pyramid, until the discovery of the real tomb, was notably similar to the Second Pyramid, in that its principal apartment contains a sarcophagus-hollow in the floor, similarly situated, and capable of receiving a stone box, firmly cemented into it, as in the second. Mr. Perring believed that in the Second Pyramid, if ever royal

* *Some say* the bones of Mycerinus, and are still maintaining it in print. But the bones in question were wrapped up in garments of *wool*—a religiously *unclean* material in Egypt, and never on any account used by them for swathing an embalmed body, where they always employed linen. But *Arabs* use wool, and said remains are rather considered to belong to an Arab workman under the Khaliphs, when they broke open that pyramid about 800 years ago. The body of Mycerinus was most probably found reduced to black dust, of which there was much inside the coffer—that being the usual result of the embalming by *spices* practised in the Old Empire of Egypt, according to William Osburn. It was only in the so-called New Empire of Thebes that the preservative powers of African *Natron* were discovered, and gave their mummies from that time forward a permanence they never had before.

burial did take place therein, that neither the chamber within which that took place, nor the real royal sarcophagus, had been found. It is still a virgin discovery for whoever shall make it: and whilst we do know, on the best evidence, that the completest security of the body after death was the thing aimed at by the Egyptians, it really is hardly for a moment to be supposed that the royal mummy should have been deposited in a sarcophagus staring unmistakeably apparent in the floor of the first-reached apartment as you arrive straight thereto from the entrance-passage,—seeing, too, that the entrance to this pyramid appears to have been a thing well understood in Egypt. Besides, history tells us further that "the body of an inferior person "was often placed in the ostensible sepulchre, to prevent any further "inquiry after the principal tomb—as in the tomb of Amasis." This arrangement evidently was intended to have been carried out, and not unlikely may have been carried out, in the Third Pyramid; because there the real sarcophagus was discovered and brought out of the real sepulchral chamber—a smaller stone box and more ornamented than the coffer in the Great, and sarcophagus of the Second, Pyramid—whereas in the chamber which is first entered, and which so well corresponds to the principal known apartment of the Second Pyramid, there exists in the floor of that chamber a hollow to receive a sarcophagus, nearly of equal dimensions to that in the Second Pyramid. In the case, then, of the Third Pyramid, we find by our own comparatively modern discoveries that secular history is to be trusted; for the very contents which that history described as enclosed therein we have found contained in it. On this view, then, we must learn to show some respect to what the same writer tells us of burial practices, and in what way burial, if it ever did take place, was carried out in the two other principal pyramids; and I need not repeat what he says as to them.

Secular history, indeed, as well as the facts, are entirely against the alleged wholly tombic character of the coffer; and in this Third Pyramid we do find that the burial could have taken place by landing the sarcophagus easily down the entrance-passage into its far-off, secret, and difficult-to-find chamber; wherein, when landed, it was afterwards made as secure as possible by ramps and blocks laid upon them in the passage leading thereto, and which Colonel Howard Vyse took out to bring the sarcophagus out of the pyramid.

But, then, modern science can well afford to make a present to the disbelievers—the so-called "advanced thinkers" of the present day—of all that secular history says; and she can also afford to let such ignorers of truth bring up all they can find in history, or by other appeal

that such men can make to any of their oracles, while she alone applies herself—to what?—the facts; when, lo, the wondrous tale she unfolds! and exhibits herself as that which she is, and must, in that day when the chaff shall be separated from the wheat, prove to be—the hand-maid of sacred history—the confirmer and expounder of much Scriptural truth. The day inevitably will come when both shall be found to agree; and never have the two made such approach before as of late, when the most apt masters of science have applied it to that "sign and wonder in the land of Egypt"—until now, to mankind a thing not understood, truly a stumbling-block to those who will not listen to its own portentous unfoldings—to those men who, despising sacred history, pretend to dictate what they consider "the reasons which experience and so-called judgment teaches," and who actually presume to define what ought to be the contents of the sacred volume of Inspiration—according to their ideas of what the Divine Spirit ought to do; and who, on reading, desire to be addressed with, not "What readest thou?" but "What thinkest thou?"

In all humbleness, we ask these men to apply science to the second, or third, or fourth, or any other of the things they call, pyramids, whether in Egypt, Ethiopia, America, or Ireland, perhaps Great Britain, or elsewhere, and let them show us in any one of them even a very rough approximation to an important mathematical quantity being involved. This they cannot do. And let Sir James Y. Simpson, or any of his school, explain in any other way they can the meaning of the special lines and angles of the Great Pyramid; let him show us what purpose the two masonry tubes leading from the King's chamber, if not for ventilation, were to serve; let him tell us, too, why that chamber is situated in the *fiftieth* course of masonry, except for again indicating an unquestionable symbolic number in the Pyramid, so oft repeated; then let him show why that despised vessel is so formed of unmistakeable proportions—these proportions indicating, amongst other things, the close approach to exactness made by man's recent researches into one of the most remarkable quantities in the whole range of physics; and let him point out any other means which will indicate so *correctly* the true distance of the earth from the sun, as the Pyramid's height does. If Sir James would apply himself to the real facts, he could then see the poverty and smallness of his attempt, for which he could never then have had occasion, to divide a great cosmical standard by the unit best known in his own language, as the "brim of his own hat."

It is, however, mere waste of time to continue in this strain, for the whole answer to the coffer difficulty, as it was formerly expressed in

regard to the whole Pyramid, is, that it tells a clear and mathematical story for itself, which a vessel of no other dimensions or proportions could, and we are contented to stand our ground until the time when the other Great Pyramid symbols, which have of late been so largely developed, shall all unite in forming that "circle of science" which in it is undoubtedly bound up.

To take another view, the question arises—Does the coffer contain any proofs of definite metrical design? Of itself it answers, Yes, unhesitatingly! Although Sir James declares that because it has turned out to be a vessel not truly rectangular in form—simply because it is not, as he terms it, "of pure mathematical form"—he makes off to the conclusion, that its *sole* end and aim was a coffin, and nothing but a coffin, for the dead body of a man. Did he, however, ever attempt to inquire whether its apparent irregularities are not really *adjustments of figure*, made purposely—designedly made—to cause that vessel to conform to its constructor's intended metrical ends? What then if we prove by actual measurements that the alleged irregularities are positively no other than such adjustments; also, if we show, out of a certain set of these roughly handled irregularities, that they were so made to typify one of the grand mathematical features represented by the linear dimensions of the whole Pyramid? But this you cannot do, says the scoffer at realities!

Restoring the coffer to its original finished state, we find, by four different modes of determining the capacity, that the mean comes out 71,250 cubic inches almost exactly*—this round number being exactly that which theory indicates, and founded upon a most important element of our cosmical system. Then this is twice repeated, because it has been again found, by reducing the apparent irregularities of

* MEAN DIMENSIONS OF THE COFFER OF THE GREAT PYRAMID, IN PYRAMID INCHES (THE LEDGE AND BREAKAGES BEING ELIMINATED).

		Length.	Breadth.	Depth.		Cubic Contents in Cubic Pyr. Inches.
1.	Inside, . .	77·85	26·70	34·31	Product — =	71 317
2.	Outside, .	89·62	38·61	41·13	Product ÷ 2 =	71 160
3.	Floor, . .	89·62 ×	38·61 ×	6·866	= 23758	= 71 266
	Sides, .	2(89·62 +	26·70) ×	34·31 × 5·922	= 47508	
4.	Inside by a 2nd method,	77·85 ×	26·70 ×	34·282	. . . . . . =	71 258
					Mean =	71 250

N.B.—The thicknesses are obtained partly from direct measures thereof, in *Life and Work*, and partly by difference of the inside and outside measures given above.

figure to their mean, that the mass of material constituting the original coffer's sides and bottom is also 71,250 cubic inches nearly; that is to say, the solid block of granite which would fill the coffer's capacity of hollow is of equal weight to the quantity of material forming the sides, ends, and bottom together. This, however, the disbelievers call "an accident," and they say that the coffer ought to have been made of equal length and breadth on its opposite sides; but holding, as the supporters of the metrical design do, that the one measure of the capacity of the hollow was intended by the constructor to also represent the cubic contents of the vessel itself, there is no difficulty in seeing that if the coffer sides had both been made equal to the maximum length of the east side, the solid contents of the vessel itself would have been larger than the solid contents of the hollow, and, on the other hand, if made equal to the smaller length of the western side, the solid contents would have been less than would fill the hollow. But the scoffer again says, the vessel could have been made with its opposite sides equal and parallel so as to constitute a rectangular vessel, and why were they so horridly misshapen? I answer, because, amongst other features, a certain significant symbol was intended to be typified by that vessel, and the maximum dimensions were absolutely necessary to produce that symbol, for, with the coffer's height, no other length and breadth but the maximum given by the bottom of the east side and north end would produce it.

The maximum length of the bottom of east side of the coffer is	90·5 inches.
Maximum breadth of north end at the bottom is	39·2 „
Maximum height at north-east corner, . .	41·3 „

It has been shown that the sum of the lengths of two sides of the base of the Pyramid is to its height as the circumference of a circle is to its diameter—or the ratio always represented by π; now, then, in the case of the coffer, we find

$$\frac{90·5 + 39·2}{41·3} = 3·1405 = \pi \text{ true within } - 0·0011.$$

Thus, by taking the *exactly corresponding sets* of dimensions in the coffer, which John Taylor first took for the whole Pyramid, we evolve the same mathematical symbol, π.

Here we see the modern measures of the coffer give π more closely than the modern measures of the Pyramid, not certainly because of greater residual amounts of error existing in that structure; but because the measures of the Pyramid itself are excessively rude and

uncertain when compared to the precise treatment of accurate measure which the coffer itself has received.*

Further, we see that it was a necessity to make the coffer of irregular figure, for if not to contain the one metrical feature, it could not, if regular, have contained the other; but it contains other important metrical features still, which, together with the two preceding, could not have been included in what Sir J. Y. Simpson calls a "mathematical vessel." Strange is it that quite recently, when measuring up the solid contents of the coffer, Piazzi Smyth found that the volume of the coffer's bottom is one half of the coffer's volume of sides and ends: this could not have been effected with the other two metrical features beforementioned in a very evenly formed vessel. Then, again, the solid contents of the mass which would fill the coffer's hollow—or, in other words, when the coffer is the very peculiarly proportioned vessel it is, the volume of the whole material forming the coffer itself—divides the cubical contents enclosed by the lower adjusted course of masonry of the King's chamber fifty times; and this chamber stands upon the *fiftieth* course of masonry of the whole Pyramid. Could all these exact metrical features occur by accidents? And is not their occurrence proofs of design? Nay, do not the scoffed-at and so-called irregularities prove themselves as purposely-made adjustments, and of necessity, to cause that vessel to conform to all the metrical features it is found to contain.

Does not the fact of the coffer's cubic contents dividing the contents of a special wall-course of the King's chamber exactly fifty times, prove that the coffer was made for that chamber. Equally, then, does the occurrence of π both in the coffer as well as the Pyramid show that *that* vessel was made for *that* Pyramid; and astonishing is not the design which secures *that* in so simple a vessel, and yet does not lose the other symbols? And when we further find that the dimensions of such vessel are founded upon the highest natural and most unvarying standard of linear measure that our cosmical system contains, pointing, too, to that most difficultly-to-be obtained quantity, "the earth's mean density," man surely should beware ere he begins to scoff and jeer

* The conviction that π was symbolized in the coffer originated itself with me about three years ago; but at that time I could not prove it with sufficient closeness, from the want of proper coffer measures. Since then, however, the real coffer dimensions have been published to the world by Piazzi Smyth, and it was only on a recent occasion, when going over these, my former conviction was recollected; and on applying it, I had the pleasure of finding it to be true. This, indeed, goes far to prove the faithfulness of those measures last brought home from Egypt.

at that which shows itself altogether of so pure, exalted, and complete a character.

To conclude this division of my subject, I have not the least hesitation in avowing, that if any honest-minded person will simply put away prejudice or previous notions, and make a fair comparison upon all the points which from time to time have been brought together on this question, he will conclude, as others in these latter days have done, that not only is the coffer no "mere sarcophagus," but that indeed the Great Pyramid, when rightly looked at, in its ancient perfect splendour, as separate and distinct from other so-called pyramidal structures, cannot be explained solely from a tombic point of view. I could even yet add further proof than what has already been given, but it is utterly hopeless to exhaust simply one feature of so vast a subject, in a single paper. So that, without touching further here upon the peculiar proportions and metrical features of this vessel, I hope to have shown abundantly that the coffer was originally some other thing than the "old, and somewhat mis-shapen, stone coffin" which Sir James Y. Simpson asserts. I pass on to a few other considerations.

Miscellaneous Comments.

It is very singular that this modern most-laboriously-worked-out metrical theory has been attacked by only two men—men certainly and justly celebrated in some respects;—these are, Col. Sir Henry James, and, as I have before mentioned, Sir James Y. Simpson. The great difficulty one feels in replying to Sir James Simpson's attack is the ascertaining of how far it is meant by him to be seriously taken, or whether it should be looked at in large amount as mere joking, for of this latter character much of it appears to be. "His mode," as it has been described by a civil engineer in London, "of representing the coffer's condition; his complaints of the errors of modern measures, as a charge against the Pyramid; his complaint of (what is just the token of design in the coffer) its unamenableness to linear measurement; and his complaint, that if linear measure were intended, the intended standard would be repeated many times all over the coffer and Pyramid, whereas it is considered by scientific men to be important *not* to repeat a standard more often than is necessary, in order to avoid either excessive error in making them identical, or else confusion from their casual differences. Again, he takes the *admitted* limits of error in modern imperfect measures of base lengths and angles, &c., and pretends that all men are agreed that these are not errors of measurement indeed, but original variations in the Pyramid. Surely all this is mere joking.

He cannot expect it to be seriously replied to by any scientific or busy man. He also picks out little mistakes, such as those of arithmetic, and even of printing—mistakes made here and there by the two or three great investigators who have elaborated the metrical theory; mistakes here and there, amid many other striking things in which there is no mistake; and mistakes which, when rectified, show the shrewdly-guessed idea in a still stronger light. Of these, we may take, for example, his comments on the coffer capacity, the Inglis base measure, the commensurability of pyramid weight with earth weight, of which the more perfect calculation only proves the comparison more perfectly. He calls up such mistakes as if the leading outline of the theory had no other supports, and as proofs of its whole fallibility; but, we may justly ask, are not such mistakes usually committed by most advocates of a new and deep theory? Such is merely and unavoidably what has been abundantly seen in all scientific progress, and is so common as not to be considered by its friends as any disgrace."

I may remind you of some mistaken arguments and data (ridiculously mistaken) raised in defence of, or in constructing some theories, which are, nevertheless, firmly held on other and independent grounds. Need I allude to whole pages of grave print and earnest argument, based on arrow-heads of flint, afterwards found to be forgeries; or on a stratum of Nile mud at least 20,000 years old, as sure as modern science exists, but in which a bit of *very modern* pottery was unfortunately discovered after all the arguments had been so nicely made out; or similar awkward mistakes discovered in caves; or to the mistakes in the best-known (so certain persons confidently say) facts in crystallography and chemistry, but which we are one day told point with absolute certainty to the igneous origin of granite, and another day to the aqueous origin of it;* or need I refer to the metrical theory, which was once said to be established and proved, as indicated by the wonderful carved tablet found in excavating a sepulchral mound within the limits of the modern city of Cincinnati, the series of lines on which were discovered to yield, in the sum of the products of the longer and shorter ones, a near approximation to the number of days in the year—a result which has been considered sufficient to ascribe to the tablet an astronomical origin, and so constituting it an ancient calendar, recording the approximation of the mound builders to the true length of the solar year. This wonderful tablet, too, has been shown to contain some

* The substance of these latter comments is abridged from a letter written by William Petrie shortly after he had read the Report in the *Daily Review* of the onset made before the Edinburgh Royal Society.

other astonishing coincidences until a metrical theory has been made out for it and yet to a great extent received by mankind—perhaps by Sir James Simpson, or at least with which we have not heard that he finds fault. Nevertheless, because the Great Pyramid contains endless coincidences which point to the completeness of its metrical character, it is most unmercifully handled and most recklessly pawed.

It is, however, really idle for me to take up time in discussing more of the objections than I have already noticed. We must remember that the Pyramid was built in the symbolic age of the world, at a time when the actions of men were ordered by symbol, and is therefore not a directly legible expression, but legible only by its own peculiar symbols; which, when rightly and fully understood, show it clearly to be a gigantic formula, an equation capable of solving numerous metrical questions that may occur in the ordinary routine of human transactions and requirements, both in a domestic, commercial, as well as economical sense. How these solutions may be effected I have not time this evening to show; but I would strongly recommend those who feel an inkling towards the truth in the Great Pyramid to examine with an unbiassed state of mind the various writers who have touched the subject.

One more point as to Sir James Y. Simpson's *dicta*, and I have done with them. Sir James, in a certain work from his pen not long since, asserts that "from the earliest historic periods in the architecture of Egypt, Assyria, Greece, and down to our own days, circles, single or double, and spirals, have formed, under various modifications, perhaps the most common fundamental types of lapidary decoration." Now, from such an assertion we should expect to find some such decoration in or about the Great Pyramid, believing it, on unquestionable evidence, to be the oldest building on the face of the earth; but most assuredly there is no such decoration in that vast pile—not a shadow even of such a thing. One little something or other there is certainly in one hitherto not yet completely explained part of the structure,—a little semiovoid chamfered projection on the northern face of the granite leaf in the antechamber leading to the King's chamber. Neither the use of this little projection nor its symbolic meaning is yet understood. At first sight it looks as if intended to constitute a projection by which the leaf might be laid hold of for being lifted, and in this sense is like projections on some Egyptian sarcophagi; but then this leaf is firmly cemented in its place, and cannot be so lifted.

Conclusion.

I may, in conclusion, bring before you one or two points which bear on this metrical theory. In the first place, the mean angle of the sides, as

shown by the more careful measurements of the casing stone angles, and deduced from the present exterior flanks of the Pyramid, is between 51° 39′, 51° 59′ and between 51° 49′ and 51° 58′, the mean of which is close upon the angle necessary for the construction of a π pyramid (namely, 51° 51′ 14·3″). But what does this peculiar angle produce? It produces in the Pyramid a most celebrated and, in such a structure, remarkable ratio, and which no other angle could produce in such a figure—the ratio of the diameter of a circle to its circumference—the mathematical quantity π. And the more remarkable feature is, that a π pyramid contains this ratio in two perfectly distinct and clear representations, each one depending on the other, and each being the direct result of the angle 51° 51′ 14·3″. The original quantity used by John Taylor for the vertical height of the Great Pyramid, as deduced from the angle of sides combined with length of base, and partly tested by direct height measures by Howard Vyse, comes out 5,832 inches = 486 feet; whilst the distance apart of the corner sockets, as ascertained by direct linear measurements over the intervening rubbish heaps, is 9,168 inches = 764 feet.* Now

$$486 : 2 \times 764 : : 1 : 3{\cdot}144; \text{ or as } \frac{1}{\pi}.$$

To get this ratio from the Pyramid, we have to perform a multiplication which does not take place in the expression for the ratio of the diameter of a circle to its circumference; that is to say, we have to multiply one base side—namely, 764 feet by 2; or in other words, we have to add together the lengths of two sides of the base before that ratio can be found. And even then, the ratio so got has not a finite look about it, because, in taking two base lengths, we only take the half of a whole quantity—that is, the half of the sum of the whole four sides of the base; for we must consider the lengths of the four rectangular base sides a whole quantity, just as we consider the circumference of a circle to be so. Since John Taylor's discovery, Piazzi Smyth has used the sum of the lengths of the four base sides as a whole quantity, which bears to the Pyramid's height the same ratio as a circle's circumference to its radius, or $\frac{2\ \pi}{1}$. These considerations led me to look for a more finite and complete expression of this π ratio without requiring any such multiplication, or without taking any half or part of what appears to be a whole

* It is exceedingly probable that whenever the true linear measure of the base is ascertained (to enable which to be accurately effected, the platform must be cleared of its rubbish), the length will come out slightly greater and produce π still more true than is obtained from the measures we now possess.

or complete quantity by itself. I was not long, then, in seeing that the meridian area of a π pyramid is to its base area as $1 : \pi$ in a direct manner, without requiring any doubling or halving whatever. We have, then, this remarkable ratio twice embodied in the mass of the Pyramid. Again, the base length is shown to be commensurable with that most unvarying, and to us important linear feature of our cosmical system—namely, the earth's polar axis, which, as we learn from the Ordnance Survey, according to the two methods for computing arc-surveys adopted, comes out, according to one method, 500,482,296 British inches, and by the other, 500,522,904 British inches—the mean value of which is 500,502,600 inches, or close upon 500,500,000. Now, Mr. Taylor has shown, and Sir John Herschel confirms, in his *Essay on the Mètre*, that the present British inch is very nearly the 500,000,000th part of the earth's polar axis; and that if it be made only 1,000th part longer, it would place our linear measure on a perfectly faultless basis. One of the grandest points, then, which the metrical theory discloses, by numerous unmistakeable coincidences, always coming out within extremely small limits of error, is that the Pyramid builders designed their work in accordance with an unit which is strikingly commensurable with this most unvarying and therefore most exalted standard; for we find that the base length of the Pyramid, as deduced from one source, comes out 9,142 inches, which being divided by 25·03, being the 10,000,000th part of the earth's polar semidiameter, gives 365·25 nearly, or the nearest whole number for the days in the solar year. It is extremely probable that the value of the solar year in measure of earth rotations will come out more exactly than the remarkably close quantity yet obtained, as there are weighty reasons for believing that this mean, 9,142 inches, is something short of the real base measure, which cannot be obtained correctly—that is to say, within the smallest possible limits of error—until the platform on which the Pyramid stands is cleared of the overlying mounds of débris there accumulated: and therefore, as before explained, such determinations as those hitherto made must always be different from the real dimensions by the amount of residual error belonging to each such attempt at mensuration under impossible circumstances for accuracy. And when we further consider that the French Academicians in 1799 give the base length as 9,163 British inches, Col. Howard Vyse as 9,168, and Mahmoud Bey as 9,162, we get another mean giving a result close to the days in the sidereal year—namely, 366·18; so that out of the various base lengths it is left to the Pyramid itself to decide which is the true one, and certainly its symbolical system does point out that one or other of the two means now taken

are very near the true base length. Here, then, we find embodied in the Pyramid—in a symbolic way, most truly, but in such a way that the curators of a metrical system could at any time make themselves sure about—the first great element of time-measure,—that element over which the French legislators in succession so grossly stumbled when they wanted to divide everything by tens, but found in the end that the unalterable time standard could not and would not be so decimalized. From this as a starting-point in time-measures, then, nearly every other subdivision of time into lunations and weeks, and so on, has of late been shown to be embodied by oft-repeated and most astonishing coincidences, in a way that cannot but impress studious minds in search of truth. That the reasons are grave indeed, and most highly suggestive of the belief that the Great Pyramid—so noble and pure a work, undefiled by any idolatry whatever—is, in chief part (included in a yet more significant design), a vast metrological monument, replete with symbols for the true guidance of man in dealing and conducting all those affairs of his nature wherein number and quantity or measure are concerned; in brief, the coffer, as well as the King's chamber, are plainly symbolic of capacity-measure, and, as dependent thereon, weight-measure also, each of which are founded on the linear measure already laid down for the Pyramid, combined with a reference to the earth's bulk and specific gravity. The present state of science shows so clearly that now is the time when the Pyramid is to be read aright by those who will attend to its most convincing, ever-recurring coincidences, pointing so unmistakeably to such a series of mathematical and cosmical facts, besides other things, that if its original purpose, in one respect at least, was not a grand material embodiment of a pure system of metrology, based upon the purest and most unvarying standard in nature, let that man come forward who is able to prove what else it is ?!

The Sun's Distance and the Great Pyramid. By W. PETRIE.

(*Communicated to the Philosophical Society through* ST. JOHN V. DAY.)

MODERN science has found the mean distance of the sun to be one of the most difficult determinations, as also it is one of the greatest and most important. Nothing less refined than the best modern mathematics and instruments can determine it with anything like scientific accuracy; and even then the error has been, within the last ten years, about 500 times as great (in proportion to the whole distance) as the

errors have been in other scientific determinations, such as the diameter of the earth.

The Great Pyramid gives this sun-distance with an accuracy which there is strong reason to believe is as perfect as can be expressed by dimensions in masonry.

One proof of this,—the most plain and practical proof,—that the continually improved results of astronomers during the last 120 years, since they found out anything about it, have continually come nearer to what the Great Pyramid fixes as its distance, thus—

	British miles.
In 1750 astronomers made it, ...	82,000,000
Early in the present century, ...	95,000,000
About 1860, ...	91,678,000
In 1867 the last and best result, by a re-computation from many former observations, but with better elements, Simon Newcomb, the American astronomer, makes the distance, ...	92,380,000

He determined this without knowing that the distance had been inferred from the Great Pyramid, in course of its being studied in England just previously in the same year, although this result was not published until after he had published his; and this Pyramid datum for the distance of the sun is 92,093,000 miles.

Modern science will not have another opportunity of trying its skill in rivalry with the Great Pyramid wisdom of primeval time (doubtless by original revelation) until the year 1882, when there will be a good transit of Venus: there will be an inferior one in 1874, not suited to give an accurate result.

The following is a brief outline of that portion of the pyramid evidence that relates to the mean distance of the sun:—

The mean distance of the sun is $10^9 \times$ pyramid altitude; that is, 1,000,000,000 times the length of the vertical axis or perpendicular height of the Great Pyramid, from its original pavement up to its original peak.

A.—Because all parts of the Pyramid constitute together a connected system of symbols relating to one primary subject. This symbol system requires, as part thereof, that the vertical height should represent the solar ray, or mean distance to the earth [and that, too, by functions of 10 and 9], so distinctly, that this was discovered not in the course of guessing, or random search for coincidences with Pyramid quantities, but by a steady process of inductive investigation of that most vital element in the Great Pyramid (its system of symbols), wherein a probability of its showing the sun's distance, as well as the

mode of it, was presented (to the writer's mind) unsought, and in the form of an unavoidable corollary to the system of symbology; and this before the last and best datum of sun-distance by Simon Newcomb had been known or calculated.

B.—Because this Pyramid, by tradition and history, has pre-eminently reference to sun and earth.

C.—Because the precise solar distance thus indicated by the Great Pyramid is the very distance that agrees best, on the whole, with all the more or less varying and uncertain determinations of the true distance which modern astronomers have made from time to time.

THE PYRAMID ALTITUDE.

The foregoing explanation depends on the altitude of the Pyramid being exactly known. We must explain that this is determined by the breadth of the base, which remains marked in the foundation rock, and by the angle of the original casing-stones, which has been exactly discovered by theory and measurement to have been such that the height is exactly the radius belonging to a circle of such a circumference as equals the length of the base, measured round all four sides; or suppose a string exactly long enough to be stretched around the sides of the square base of the Pyramid, and then this string spread out on a flat piece of ground so as to be in a circle, instead of a square form, the radius of that circle, set upright, would be the height of the Pyramid. This height is 5,835 British inches.

THE BASE BREADTH

Is determined by the weighted mean of the careful measurement of the French Academicians, with the most perfect instruments that were then known, and the measurement of Colonel Howard Vyse, which were found in other places to be very correct. This mean is 9,165·59 British inches.

There are also other considerations, which cannot here be entered on, which determine this matter to be exactly the same as the above mean measurement.

That a structure, executed at so great a cost, and such as would be in many ways the greatest triumph for human labour, skill, and science, should be made ostentatiously devoid of inscription, or of any other mode of recording any man's exaltation, is itself a strong corroboration of its design having been given by supernatural command, like that of the ark, the tabernacle, and temple of Jerusalem; for it is

quite contrary to what we know of human nature, and more particularly contrary to the ways of men in such times and places, as it is emphatically testified by the adjacent catacombs, that so magnificent, so unparalleled, and eminently warrantable an opportunity should not be taken for exalting man, and especially the originator, owner, and ruler of the work, and that by aid of inscriptions, if he were a mere man.

The above-named peculiarity reminds us, not of any of the great humanly-devised structures, from the Pyramids themselves to St. Paul's Cathedral, London (though devoted to God), replete with inscriptive exaltations of man; but in this respect the Great Pyramid reminds us of the divinely-commanded temple at Jerusalem, to the one God over all.

APPENDIX.

Notes on Structures called Pyramids.

SINCE the foregoing paper was read to the Philosophical Society, it has been represented to me, as advantageous to the subject, to supplement it with some considerations on other structures which some persons have called pyramids. The following notes, and accompanying wood, as well as plate, engravings of a few examples of such structures, have been put together in the hope of tending, to some extent, to remove a cause of much confusion to latter-day readers. There can be no question that the mischief which has been produced by such an universal application of one term to numerous varieties of structures, originally raised for as many different purposes, is the cause of much of that unhappy dogmatism which now-a-days exists, in persisting to say that the Great Pyramid is nothing else than a tomb. An idea and belief solely tombic having got possession of the minds of most readers, being what they were taught by people of the passing generation, in whose day the definite metrical peculiarities, and therefore realities, of the Pyramid were unknown, is naturally and persistently held, simply because so comparatively few minds have yet the courage to make the beginning to try and examine if there is not a greater foundation of reality in what the most refined applications of science have since their school-days discovered, than in what they were then taught: yet, whilst a solely tombic theory has nothing convincing about it, because there is nothing to prove it; yet the metrical (shall we say *symbolic ?*) theory, because it is of recent birth, and does contain endless series of unmistakeable symbols pointing to such high and lofty ends, and requiring efforts of a loftier order

than hitherto considered necessary to understand the Pyramid, half the world is afraid to give itself the exertion of really ascertaining what the metrical features are; and because they have not courage to do so, such persons presume to condemn those who have sought to understand its exquisite symmetry and purity; nay, they even condemn the mighty structure itself.

I shall not enter into any descriptive statement as to the Great Pyramid, nor of any of the Jeezeh Pyramids, because all this is so excellently done in a collected and easily accessible way elsewhere. The accompanying woodcut (Fig. 1) will, however, demonstrate one feature peculiar to the Jeezeh group—namely, their universal similarity

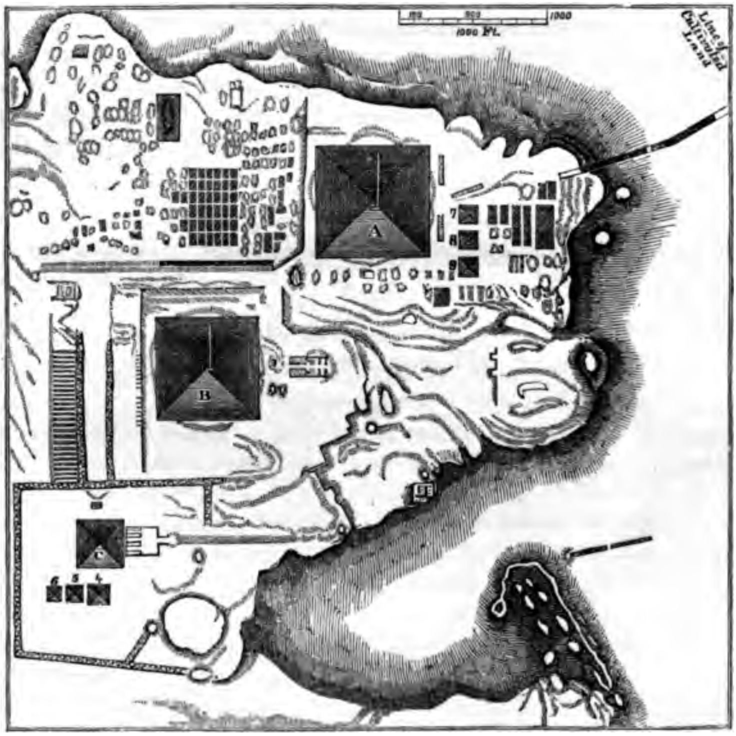


Fig. 1.

of orientation. I take it for granted that the well-recognized internal splendour and structural magnificence of the Great Pyramid are both well understood; also that not even another Jeezeh, or other Egyptian pyramid, resembles it, except in that they are entered by passages similarly situated, but of vastly inferior struc-

ture, to its entrance passage. None of them contain any chambers, except such as are subterranean, and in no way symmetrical. They are all of different proportions, and all smaller than the Great Pyramid. Yet Sir J. Y. Simpson indeed declares that it is just like any of the other pyramids—Irish, American, &c., &c. He brings up the New Grange tumulus, and pretends to compare this with the majestic Jeezeh mass; and endeavours to add weight to his argument by quoting from Sir William Wilde, who, in writing on the Boyne tumulus, and comparing it with the pyramids he had seen in Egypt, says, "*the type and purpose is all the same.*" I strongly suspect this idea Sir William Wilde borrowed from Governor Pownall, who described this barrow in the *Archæologia.*

I have purposely taken some pains to represent this structure, on which so much dependence appears to be placed, so that we may not fail to discover the similarity and identity, if such exist; for it is declared that their oneness of purpose does exist. Let us see, then, if we can find it. Fig. 2 is a section of "the Great Pyramid of the

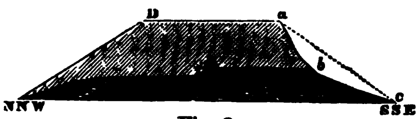


Fig. 2.

Boyne," whilst Fig. 3 is a plan of the same notable structure. It is well to remark its trapezoidal base, and the position of the passage leading

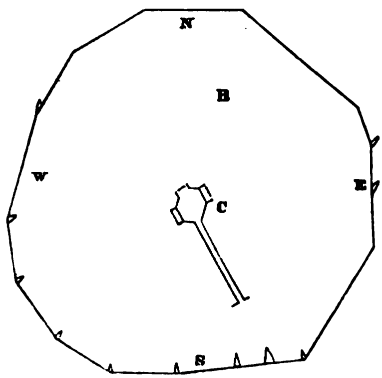


Fig. 3.

to the sepulchral chamber, the position of the three niches, and compare the whole with the four cardinal points of the compass; and where do we find an approach to orientation, or even a regular, rectilinear, geometrical figure at all? Where, indeed, is its similarity to the Great

Fig. 4.

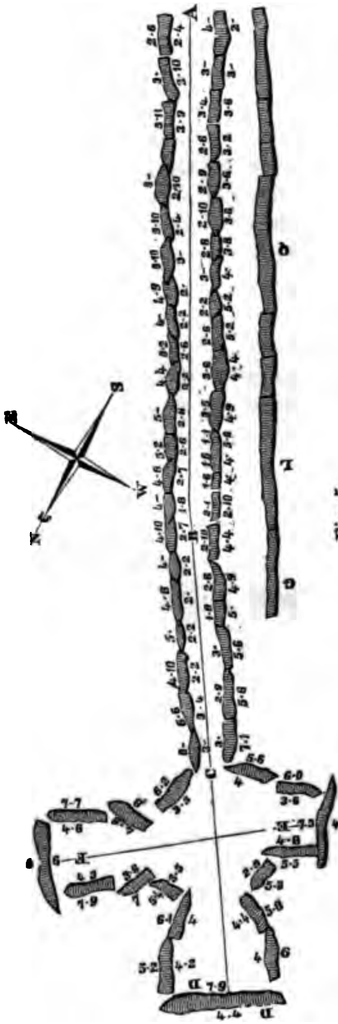


Fig. 5.

Fig. 6.

Fig. 7.

Pyramid of Jeezeh? We may perhaps find it in the gallery, or the sepulchral chamber, not improbably in the general structure of its mass of solidity. Let us, then, look at its gallery and chamber in sectional elevation looking east, Fig. 4, and in plan, Fig. 5; and then we may consider one of the rudely, somewhat oval-shaped stone pans or basons shown at Figs. 6 and 7; and is there a feature like the Great Pyramid, or even anything nearly so good as the very worst pyramidal mound that has been found anywhere throughout the Nile valley? This Irish tumulus was very elaborately explained about the year 1770 A.D., by Governor Thomas Pownall, who wrote a creditably complete paper concerning it, and expended much money and time in having it properly surveyed by a Mr. Bowie, under the direction of Dr. Norris, "master of the great school of Drogheda," and from whose measurement and examination a complete set of drawings were made. The paper and engravings from these drawings were brought before the Society of Antiquaries of London in June, 1770; and from these drawings, published in the *Archæologia*, vol. ii., I have prepared the illustrations, whilst the following extracts respecting this tumulus are transcribed from the venerable pages of that publication.*

(Page 238.) "Accident in like manner about the end of the last century discovered an opening in the side of the *Great Pyramid at New Grange* † in Ireland; and this aperture, by a gallery, led to a cemetery composed of three tabernacles or niches in the centre of the base."

Now, this is the first clue that I am able to find wherein the tumulus (notably mentioned in the title of the paper as a "Sepulchral Monument," and spoken of in the early parts of the paper as one of the "tumuli" or "mounds" of earth raised over the bodies of great and famous persons—called in south-eastern countries *Bougors*, and by us Burrows or Barrows) is so ridiculously dignified with a name even approaching to "*Pyramid*," to say nothing of ascending to the distinction of being prefixed by the adjective "*Great*." No doubt Sir James Y. Simpson, in this paper of Governor Pownall's, found it so called, and as such a name applied certainly with some show of authority to such a structure—comes in most fittingly for his attack on the character of the noble Monument at Jeezeh—it was indeed no doubt hard to leave alone the temptation of using (what seemed to him) so powerful an unit in his assault.

* The title of this paper, which is well worth careful reading, is, "A Description of the Sepulchral Monument at New Grange, near Drogheda, in the County of Meath, in Ireland." By Thomas Pownall, Esq., in a letter to the Rev. Gregory Sharpe, D.D., Master of the Temple.

† These italics are mine.

(Page 250.) "As most, if not all, the Barrows which we know of (a few small carneddas excepted) are formed of earth, you will, upon your approach to this, be surprised to find it a pyramid of stone, compiled of *pebble* or *cogle* stones, such as are commonly used in paving. But what conceptions must we have of the expense of labour and time, and of the number of hands necessary to such a work, when we understand that these stones must have been brought hither not less than twelve or fourteen miles from the sea-coast, at the mouth of the Boyne! Such materials lie there; but I am assured, by gentlemen who know the country where this monument is erected, that there are no such stones as it is composed of to be found within land. When I add to all this, that, upon a calculation raised from the most moderate state of its measurements, the solid contents of this stupendous pile amount to 189,000 tons weight of stone, your astonishment must, I think, be raised to the highest pitch."

Language of the kind now quoted is almost certain to inflame the mind of any man not accustomed to works in masonry, earth, and so forth, nor used to handle large round masses of figures, with a picture immensely extravagant and wide of the true nature of things; and to any one setting about an assault like that of the Edinburgh medical baronet—who, from his busy professional occupation, can hardly have the necessary time, and but few opportunities of practising that special kind of observation so necessary for the real appreciation of ancient as well as modern practice in the structural arts—certainly this dashing language of Governor Pownall's comes most opportunely for his purpose; but it loses its force when we find this "pyramid of stone" is "compiled of pebble or cogle-stones." Let it be remembered that a really *pyramidal* structure, by which use of the adjective I mean a solid terminating in an apex, unless extremely flat, could never have been erected of such material. I should say, with a much flatter angle of side than that shown by Fig. 2; as, when approaching the apex, the weight of the uppermost layers of stones, having but so small a surface of support compared to that of the under layers, and the inclination of such structure being so much in excess of the angle of repose of the material (piled together loosely, as it is reported to be), the tendency and direct result of the uppermost part, if such ever existed, would be to slide down the sides and accumulate round the base, thus making the angle of side even less than at first (supposing for the moment that the structure was originally apiculated), we may therefore safely conclude that from the practical difficulty, nay, impossibility, of making such a structure terminate with its angle of side in an apex, it never could have so terminated, and therefore never existed in any state far different from the extremely rude approximation to a conic frustum which it now presents.

(Page 253.) "Fig. 3 gives the plan of the base drawn according to Mr.

Bowie's stations in measuring it; but you must understand that the periphery of the real figure is curvilinear, not rectilinear. This base covers about two acres of ground. C is the plan of the cave and the gallery leading to it; as it bears 24° N.W. Fig. 3 is the section of the pyramid, and of the ground on which it stands, projected from a medium of the various numbers I have received. The whole is laid down by a scale of 84 feet to an inch."

(that is, 168 feet to an inch in the illustration now given by me—these being one-half the size of those by Governor Pownall).

"This pyramid was encircled at the base with a number of enormous unhewn stones set upright, of which ten were remaining when I was on the spot. These you will see marked in the plan. Nine of them are still in their erect position, the tenth is thrown down. I measured many of these stones, and found them from 7 to 9 feet high above ground; that which is thrown down, and lies quite out of the ground, measured near 11 feet. Their forms are various and anomalous. Upon a rough estimate they may be supposed to weigh from eight to twelve tons each. Mr. Lhwyd says there was a stone of considerable bulk erected on the summit of this pyramid, of the same anomalous form as the others, but of less size. But there were no remains of such when I was there.

"The pyramid in its present state is, as I said, but a ruin of what it was. It has long served as a stone quarry to the country round about. All the roads in the neighbourhood are paved with its stones: immense quantities have been taken away. Mr. Lhwyd mentions the particular instance which gave occasion to the discovery of the gallery that leads to the cemetery. The mouth of this gallery, under the perfect state of the monument, lay concealed and shut up near 40 feet within the body of the pile. The dotted line *a c*, in the section, Fig. 2, gives the supposed perfect side. The triangle *a*, *b*, *c* is the hollow space from whence, as from a stone quarry, the stones have been taken; *b* marks the mouth of the gallery. This gallery is formed by large flag stones. Those which compose its sides are set on edge, and are of different altitudes, from 2 to 7 feet high, and of various breadths from 2 to 3 feet 6 inches, as may be seen by the figures in the plan Fig. 5, where the figures on the outside denote the altitude of the stones, those on the inside their breadth. The thickness of each could not be taken with any certainty; but some of the large ones which form the cemetery are from 1½ foot to 2 feet thick.

"Fig. 4 gives a section of the gallery, and of the east tabernacle or niche in the cemetery. Fig. 8 is a perspective section of the north side opposite to the entrance.

"One of the stones, marked Q, Fig. 5, which lies across, and forms part of the top or roof of the gallery, is 13 feet long and 5 feet broad; another at L is 11 feet long and 4 feet 6 inches broad.

"This gallery at the mouth is 3 feet wide and 2 feet high. At 13 feet from the mouth it is only 2 feet 2 inches wide at the bottom, and of an indeterminate width and height. Four of the side stones, beginning from the fifth on the right hand, or eastern side, stand now leaning over to the opposite side; so that here the passage is scarce permeable. We made our way by creeping on our hands and knees till we came to this part. Here we were forced to turn upon our sides, and edge ourselves on with one elbow and one foot. After we had

passed this strait we were enabled to stand; and by degrees, as we advanced farther, we could walk upright, as the height above us increased from 6 to 9 feet. The distance from A to B in the ground plot, Fig. 5, is 42 feet; from B to C is 19 feet 4 inches; from C to D, 19 feet 2 inches; from E to F, 21 feet. You will observe from the Plan that, although the cemetery is

Fig. 8.

an irregular polygon, yet it is such an octagon as might be supposed to be formed with such rough materials into so rude a style of architecture. The dome of this cave or cemetery springs, at various unequal heights, from 8 to 9 and 10 feet on different sides, forming at first a coving of eight sides. At the height of 15 or 16 feet, the north and south sides of this coving run to a point like a gore, and the coving continues its spring with six sides; the east side coming to a point next, it is reduced to five sides; the west next; and the dome ends and closes with four sides, not tied with a keystone, but capped with a flat flag stone of 3 feet 10 inches by 3 feet 5 inches. The construction of this dome is not formed by keystones, whose sides are the radii of a circle, or of an ellipsis converging to a centre. It is combined with great long flat stones, each of the upper stones projecting a little beyond the end of that immediately beneath it, the part projecting and weight supported by it bearing so small a proportion to the weight which presses down the part supported, the greater the general weight is which is laid upon such a cove the firmer it is compacted in all its parts. This will appear without any further explanation, from a bare inspection of Figs. 4 and 6.

"The eight sides of this polygon are thus formed. The aperture which forms the entrance, and the three niches or tabernacles, make four sides, and the four imposts the other four. Upon the whole, this cemetery is an octagon with a dome of about 20 feet in height, and of an area which may be circumscribed within a circle of 17 or 17½ feet. Fig. 8 gives a view of the tabernacle opposite to the entrance, as Fig. 4 does of the east side one. I will begin my description with that on the east, or right hand. Each side of this consists of two stones standing erect, in the position and of the dimensions as marked in the Plan, Fig. 5. The back is formed by a large flat stone laid edgeways at its length; its position and dimensions are also marked in the same Plan. The whole is covered with one large flat stone, sloping towards the back, and thus forms what, in the language of the old

British inhabitants, is called a Kistvaĕn. The northern tabernacle is constructed exactly as the eastern one. The other on the western or left hand side differs, each side of it being composed but of one single stone, as may be seen in the plan. Where the back stone does not reach quite up to the top covering stone, there the space is completed by a kind of masonry of three courses. The northern tabernacle hath for its floor a long flat stone, 6 feet 8 inches long by 4 feet 11 inches broad. The two side niches have no other floor but the natural ground. They have, however, each of them, a rock basin placed within them. That in the left-hand niche stands on the natural ground. That on the right is placed upon a kind of base. It appeared to me, when I made my sketch, rather convex than as it is described by others, and as given to me by Mr. Bowie. But herein I may have been misled by the earth which lay about it. As this basin seemed to have the sides of its concave fluted, I desired particularly that the surveyor might clean it and wash it; that if there was anything singular it might be observed. Nothing particular was found there; so take the draught just as I first sketched it. The basin on the right hand, as the surveyor gives me the measure, is 4 feet 9 inches by 3 feet 4 inches; as I measured it, it is 3 feet 11 inches by 3 feet 5 inches. The surveyor's measure of the base is 6 feet by 5 feet 4 inches. The basin in the left-hand tabernacle is exactly of the same form as the other; its dimensions 4 feet 4 inches by 3 feet 7 inches. In the narrow point of its oval it is 2 feet broad. Dr. Molineux, in his account of this cemetery, says that there was a rock bason in each niche; and as that stone which I have described as a base is a concave, forming a basin like the rest, it may at the first view seem to give some foundation for this account. But Mr. Lhwyd says expressly, 'that in each cell or apartment on the right and left hand was a broad shallow basin of stone; the basin on the right hand stood within another; that on the left hand was single; and in the apartment straight forward there was none at all.' As this account was prior to the Doctor's, and as both the drawing and Plan from which the Doctor wrote describe this base-stone (which one might suppose to be the third basin) as actually then standing as a base to the right-hand basin, it is clear that the Doctor was mistaken; and indeed a bare view of the inaccurate plan from which he wrote his description shows how that mistake arose."

Thus far, then, we have no difficulty in fully understanding what sort of thing this New Grange affair is, which has recently been set up, with such a sham show of authority, as a rival to the Great Pyramid of Jeezeh, and I am sure that it is needless to say another word concerning it; although for those who, not knowing what they really do, in striving to tear down to the foundation that truth bound up in the Great Pyramid,—to such it may answer their purpose to classify two things of so totally dissimilar character.

When we consider that the earliest monuments in Europe were mounds of earth, or cairns of stones raised by some Turanian offspring over the remains of the dead; and knowing that, in very early instances, they were left simply rough and apparently unfinished, the New Grange monument, from the circle of basement stones, appears to be of not earlier than Etruscan date. The Etruscans im-

proved upon the older tumuli by surrounding the base with a podium, or supporting wall. The Etruscan tumuli appear to be always circular; still they have been, and are yet being persistently, called pyramids; as in the case of the Regulini Galeassi tomb, and the Cocumella Vulci, whilst in Asia Minor the same order of tumulus is found as at Tantalais on the north shore of the Gulf of Smyrna. The New Grange mound is altogether a most inferior example of a tumulus: there are many others in Europe—yea, even in our own British Isles—of far higher structural type.

American Pyramids.

Passing from Europe to America, we find quite another order of things there called pyramids. Besides the various sepulchral tumuli of earth and loose stones in various parts of that country, there are to be found several huge masonry erections, two of which are shown at Plates V. and VI.—namely, the Pyramid of Oajaca at Tehuantepec, and the Teocalli at Palenque.

In Stephen and Catherwood's *Central America*, 1854, the distinctive character of these American pyramids is thus mentioned, p. 529:—

"The pyramids of Egypt are known to have interior chambers; these (namely, the American) are of solid earth and stone. No interior chambers have ever been discovered, and probably none exist. And the most radical difference of all is, the pyramids of Egypt are complete in themselves. The structures of this country (America) were erected only to serve as the foundations of buildings. There is no pyramid in Egypt with a palace or temple upon it; there is no pyramidal structure in this country without,—at least, none from whose condition any judgment can be formed."

Fergusson in his *History of Architecture* remarks:—

"The principal monuments of the valley are the Teocallis—literally, Houses of God—the Temples of the people. These are pyramids in terraces, with flat tops, and always surmounted by a chamber or cell, which is in fact in the temple itself. They seem to be of all ages, for, if one may trust the tradition, that of Cholulu is as old as the early Toltecs, whereas the great Teocalli of the city of Mexico was only finished five years before the discovery of America by Columbus, and the Spaniards met with many persons who had assisted in its erection. It has, however, with all the native buildings of the city, been swept away by the ruthless bigotry of the conquerors. . . .

"Of these teocallis, the largest, probably also the oldest, is that of Cholulu. Its dimensions, in so far as it can be ascertained in its present ruinous state, are 1,440 feet square, and 177 feet in height, divided in four storeys, the fifth being formed by the cell or temple, which has now been replaced by a chapel dedicated to the Virgin Mary. The whole is composed of badly burnt bricks and mud, and is now so overgrown with trees, that it is difficult to make out its form; but in Humboldt's time it apparently was freer from obstruction and more easily traced.

"There are two pyramids at Teotihuacan, the largest of which is apparently a square of 645 feet, with a height of 171 feet; and there are others at Tezcuco of about the same dimensions, and, like them, divided into five or seven storeys, but the most interesting of those yet brought to light is that of Xochicalco. It is situated on the top of what appears to be a natural elevation, but which has been fashioned into terraces by art. The pyramid itself is in five storeys. . . .

"Besides these great many-storeyed pyramids, there are numerous examples in various parts of the country of one storey only; several of these have been described, but unfortunately not drawn. Their general arrangement may, however, be judged of from the example from Oajaca, Plate V. Like all others in Mexico, it is only a device to raise a temple to such a height as should give it dignity, and enable the ceremonies performed on its upper platform to be seen by all the people.

"It is indispensably necessary to bear this distinction in mind in speaking of these monuments, as careless writers, connecting the word pyramid with Egypt, have been too apt to confound together two classes of monuments entirely distinct and dissimilar. . . . . In no instance (of Egyptian pyramids) are there external steps leading to a cell or chamber on the apex. . . . . . The Assyrian pyramids, on the contrary, have much more affinity with the buildings of which we are now speaking. They were always in terraces, the upper platform was always crowned by a chamber or cell, and there were external steps leading to this, which was the principal object of the erection. We have traced this form of temple to the shores of the Eastern Ocean. If we still, however, hesitate to pronounce that there was any connection between the builders of the pyramids of Suka and Oajaca, or the temples of Xochicalco and Boro Buddor, we must at least allow that the likeness is startling, and difficult to account for on the theory of mere accidental coincidence. . . . .

"As in Mexico, the principal monument of Yucatan is the Teocalli. In the latter province, however, they seem to differ somewhat in design from those above described. They are not generally in terraces, but rise, at an angle of about 45°, to the level of the platform on which the temple stands; and a magnificent unbroken flight of steps leads from the base of the building to its summit. Almost all these retain more or less of the remains of architectural magnificence that once adorned their summits. The annexed Plate, VI., representing the elevation of a temple at Palenque supported by a pyramid, will give a good general idea of their form. The pyramid is about 280 feet square, and 60 feet in height: on the top of it stands the temple, 76 feet wide in front, and 25 feet deep, ornamented in stucco with bassi-relievi of better execution than is usually found in these parts, and with large hieroglyphical tablets, whose decipherment, were it possible, would probably reveal to us much of the history of these buildings.

. . . .

"The other temples found in Yucatan differ but little from this one, except in size, and, architecturally speaking, are less interesting than the palaces—the splendour of the temple consisting in the size of its pyramid, to which the superstructure is only the crowning member: in the palace, on the other hand, the pyramid is entirely subordinate to the building it supports, forming merely an appropriate and convenient pedestal, just sufficient to give it a proper degree of architectural effect."

Ethiopian Pyramids.

At several spots scattered on the banks of the Upper Nile we find various structures, many of them somewhat pyramidal in form, yet few, if any of them, bear any traces of ever having been finished in a sharp apex at the summit: on the contrary, all of these concerning which I can gather information, bear very decided significance of having terminated in *a flat* top,—an architectural finish unknown in any purely Lower Egyptian Pyramid. Whilst the Meroë Pyramids belong to a unique and distinct order, their inferiority of construction is particularly remarkable, the pyramid part of the building being in most cases nothing more than a heap or pile of loose and rough material covered by a casing of wrought stone, the latter frequently very uneven and badly jointed. They are also remarkable in being finished with a pronaos or porch, sometimes of very large dimensions, while frequently much smaller, as shown at Plates VII., VIII., and IX.; the pronaos constituting the receptacle for the dead, and therefore the only really useful part of the structure. The Meroë Pyramids have been pitched down wherever it seems to have pleased the builder, as may be gathered from the plans in Plates VII. and IX., none being oriented; and each constructor appears to have adopted whatever form or finish he chose. Judging from the drawing in Lepsius's *Denkmaeler*, some of them are much ornamented on the exterior, some plain, and others apparently unfinished, whilst a peculiarity appears to exist in the case of a roll moulding at all the angles, this in some cases being cut on the outer end of the casing stones, as shown at Fig. 1, Plate VIII. In others the angles are composed of prisms of stone let into the angular space formed at the junction of two surfaces of casing stones, as shown at Fig. 2, Plate IX.; whilst in others the flat surfaces of the casing stones themselves are ornamented with a raised panel, as shown at Fig. 3, Plate IX., which represents part of a casing stone surface, and in which the unevenness of the joints is plainly seen. Again, some of these Meroë buildings are formed of stepped surfaces, as shown at Fig. 4, Plate IX., each layer of casing stones receding inwards from that immediately below it, but with the sides inclining, although to a less degree than the actual inclination of the whole side of the structure itself.

Speaking of Ethiopian structures, Fergusson, in his *History of Architecture*, says:—

"The most remarkable monuments of the Ethiopian kingdom are the pyramids, of which three great groups have been discovered and described. The principal group is at a place called Dankelah, the assumed site of the

ancient Meroë, in latitude 17° north. Another is at Gibel Barkal; the third at Nourri, a few miles lower down than the last named, but probably only another necropolis of the same city.

"Compared with the great Memphite examples, these pyramids are most insignificant in size,—the largest at Nourri being only 110 feet by 100; at Gibel Barkal the largest is only 88 feet square; at Meroë none exceed 60 feet each way. They differ also in form from those of Egypt, being much steeper, as their height is generally equal to the width of the base. They also all possess the roll moulding on their angles, and all have a little porch or pronaos attached to one side, generally ornamented with sculpture, and forming either a chapel, or more probably the place where the coffin of the deceased was placed. We know from the Greeks that, so far from concealing the bodies of their dead, the Ethiopians had a manner of preserving them in some transparent substance, which rendered them permanently visible after death.

"To those familiar with the rigid orientation of those of Lower Egypt, perhaps the most striking peculiarity of the pyramids is the more than Theban irregularity with which they are arranged, no two being ever placed, except by accident, at the same angle to the meridian, but the whole being grouped with the most picturesque diversity, as chance appears to have dictated.

"Among their constructive peculiarities it may be mentioned that they seem all to have been first built in successive terraces, each less in dimensions than that below it, something like the great pyramid at Saccara, these being afterwards smoothed over by the external straight-lined coating.

"Like the temples of Gibel Barkal, all these buildings appear to belong to the Tirhakah epoch of the Ethiopian kingdom. It is extremely improbable that any of them are as old as the time of Solomon, or that any are later than the age of Cambyses, every indication seeming to point to a date between these two great epochs in the connection of African history with that of Asia."

Assyrian Pyramids.

Most probably owing to the forms of some of the ruinous piles situated on the banks of the Euphrates approximating to that which a true pyramid in a state of far-gone dilapidation and decay would, and in many cases even of the Egyptian pyramids, has assumed,—has the word been applied to several proto-Chaldean, as well as later Chaldean, structures. These structures, although for long years mere shapeless masses, we know now, by the light which has been shed upon them by the comparatively recent investigations of Rawlinson, Layard, Loftus, and Taylor (although in no case does it appear that anything has been found which renders quite certain our restoration to the original architectural forms entirely), were chiefly temples situated on the top of vast terraced blocks of brick-work, as in the case of the Birs Nimroud and the Mugheyr. It is needless to add that some of these structures were highly ornamented, and contain many points of strictly architectural and no doubt capricious finish, totally devoid of anything that can scientifically be considered pyramidal. Indeed, they

are entirely non-pyramidal, unless "pyramid" be taken to mean (which it certainly ought not) a heap of any form, composed in any possible way, between the limits of the rudest burial-mound of the wildest savage, up to the unequalled masonry of the profoundly meaning Jeezeh pile.

The remains of the earliest proto-Chaldean temple can scarcely be of remoter date than about 1900 B.C.; and the chief cause why it is so difficult to form a correct representation of the old form of temple is, that the material of which most were composed was either sun-dried or very imperfectly burned brick. The later Chaldean temples, of date about 600 B.C., and up to the time when Babylon was taken by Cyrus, appear to have been constructed of a better class of bricks; but vast quantities of them have been quarried out of the older edifices and used in the construction of other buildings, raised by succeeding occupants of the country, according to their caprice and taste. To a large extent we owe our own ideas of what these Babylonian structures were to the copying of some of them in stone by the Persians in their own country, as in the case of the alleged tomb of Cyrus at Passargadœ.

Speaking of these structures, Fergusson, who compiles his information from the writings of Rawlinson, Loftus, Layard, and Taylor, says:—

"The oldest temple we know of at present is the Bowariyeh at Wurka (Erek), erected by Urukh, at least 2,000 years B.C.; but now so utterly ruined, that it is difficult to make out what it originally was like. It seems, however, to have consisted of two storeys at least: the lowest about 200 feet square, of sun-dried bricks; the upper is faced with burnt bricks, apparently of a more modern date. The height of the two storeys taken together is now about 100 feet; and it is nearly certain that a third, or chamber storey, existed above what now appears.

"The Mugheyr Temple is somewhat better preserved, but in this case it is only the lower storey that can be considered old. The cylinders found in the angles of the upper part belong to Nabonidus, the last king of the later Babylonian kingdom; and the third storey only exists in tradition. Still, from such information as we have, we gather that its plan was originally a rectangle 198 feet by 133, with nine buttresses in the longer and six in the shorter faces. The walls slope inwards in the ratio of 1 in 10. Above them was a second storey, 119 feet by 75, placed, as is usual, near one end of the lower storey, so as to admit of a staircase being added at the other. It is 47 feet distant from the south-eastern end, and only 28 or 30 from the other; but whether the whole of this was occupied by a flight of steps or not is by no means clear. Taken altogether, the plan and probable appearance of the building when complete may have been something like that represented in Plate X., though there are too many elements of uncertainty to make it a restoration which can altogether be depended upon.

"The typical example of this class of temples is the Birs Nimroud.

It is true that as it now stands every brick bears the stamp of Nebochadnassar, by whom it was repaired, perhaps nearly rebuilt; but there is no reason for supposing that he changed the original plan, or that the sacred form of these temples had altered in the interval. It owes its more perfect preservation to the fact of the upper storey having been vitrified after erection by some process we do not quite understand.* This now forms a mass of slag, which has to a great extent protected the lower storeys from atmospheric influences.

"In so far as it has been explored, the lower storey forms a perfect square, 272 feet each way. Above this are six storeys, each 42 feet less in horizontal dimensions. These are not placed concentrically on those below them, but at a distance of only 12 feet from the south-eastern edge, and consequently 30 feet from the N.W., and 21 feet from the two other sides.

"The height of the three upper storeys seems to have been ascertained with sufficient correctness to be 15 feet each, or 45 feet together. Unfortunately no excavation was undertaken to ascertain the height of the lowest and most important storey. Sir Henry Rawlinson assumes it at 26; and I have ventured to make it 45, from the analogy of the tomb of Cyrus and the temple at Mugheyr. The height of the two intermediate storeys, instead of being 22 feet 6 inches, as we might expect, was 26, which seems to have resulted from some adjustment due to the chambers which ranged along their walls on two sides. The exact form and dimensions of these chambers were not ascertained, which is very much to be regretted, as they seem the counterpart of those which surrounded Solomon's temple and the Viharas in India, and are consequently among the most interesting peculiarities of this building.

"No attempt was made to investigate the design of the upper storey, though it does not seem that it would be difficult to do so, as fragments of its vaulted roof are strewed about the base of the tower-like fragment that remains, from which a restoration might be effected by any one accustomed to such investigations. What we do know is that it was the cella or sanctuary of the temple. There probably also was a shrine on the third platform.

"This temple, as we know from the decipherment of the cylinders which were found on its angles, was dedicated to the seven planets or heavenly spheres, and we find it consequently adorned with the colours of each. The lower, which was also richly panelled, was black, the colour of Saturn; the next, orange, the colour of Jupiter; the third, red, emblematic of Mars; the fourth, yellow, belonging to the sun; the fifth and sixth, green and blue respectively, as dedicated to Venus and Mercury; and the upper probably white, that being the colour belonging to the moon, whose place in the Chaldean system would be uppermost. A general idea of this structure may be gathered from Plate XI.

"Access to each of these storeys was obtained by stairs, probably arranged as shown in the plan, Plate XI. These have crumbled away or been removed, though probably traces of them might still have been found if the explorations had been more complete.

"Another temple of the same class was exhumed at Khorsabad some ten or twelve years ago by M. Place, but his drawings are still unpublished. It

* Fergusson says vitrified. It appears more probable that they may have been cemented together with bitumen, as in the case of the walls of Babylon.

consisted, like the one at Borsippa, of seven storeys, but in this instance each was placed concentrically on the one below it; and instead of stairs on the sloping face, a ramp wound round the tower, as we are told was the case with the temple of Belus at Babylon. The four lower storeys are still perfect. Each of them is richly panelled and coloured as above mentioned, and in some parts even the parapet of the ramp still remains *in situ.*"

It was my intention to have made these notes on the various structures which are called pyramids much more complete, but the necessity of not delaying publication of the Philosophical Society's Transactions has left me with insufficient opportunity for extending them. However, the foregoing remarks and quotations will, I believe, be sufficient to render intelligible the accompanying plates, and I hope be of use in tending to clear up some of the confusion which appears to exist, as to what structures should and should not be considered pyramids. In bringing the various classes of structures thus together, the main object has been to enable comparisons to be easily made, and after such have been accurately carried *through*, let men pronounce their verdict on the open question, "whether that which satisfactorily and undoubtedly explains every other so-called pyramidal structure, can as satisfactorily and completely explain the great Pyramid of Jeezeh? and if not, what other meaning than one somewhat tombic, but chiefly metrical, and pointing again to something still higher, does so explain it?"

III.—*On a new Plastic Material.* By MR. EDMUND HUNT.

Read March 18, 1868.

IT having occurred to Mr. John Clark, jun., of Mile-end, Glasgow, that sawdust, hitherto looked upon very much as a waste product, might be converted to some useful purpose, that gentleman has recently been at the trouble and expense of some experiments with that material, and has obtained very promising results. Instead of patenting the process, his wish is to make it public. He has therefore requested me to lay before you the results he has obtained, in the hope that you may find them of sufficient interest to warrant this communication, and in the hope also that some of you may be able to make further use of what little information he has been able to gather. Mr. Clark's idea has been, if possible, to reproduce the concrete condition originally possessed by the sawdust as wood, and he has succeeded in

attaining his object by combining the sawdust with another substance, and in some cases with more than one other substance. In this way a mixture has been obtained which is plastic or semifluid, at a sufficiently elevated temperature, so that it can be cast or moulded into any desired form, but which is at ordinary temperatures quite hard, dense, and tough.

The principal substance which Mr. Clark employs in order to effect the amalgamation or concretion of the sawdust is sulphur. The sawdust is taken either as it comes directly from the saw, or it is first converted into charcoal, and melted sulphur is added to it in quantity just sufficient to saturate it. It is then pressed and allowed to cool, when it forms a strong coherent material, capable of being applied to a variety of purposes. About a pound and a half of sulphur has been found sufficient for each pound of sawdust. The compound must be heated to a temperature of at least 235° when it is about to be moulded into any desired form; and it should be heated in a cylinder fitted with a piston, so that a sufficient pressure may be brought to bear on it to force it, by a suitable outlet, from the cylinder into the mould. The mould is preferably made of cast iron, and it should be filled as quickly as possible, in order that the material may not cool before it reaches every part of the mould.

Bitumen (asphalt) or common rosin may be substituted for a portion of the sulphur; but when ingredients are included that have higher melting points than that of sulphur, a correspondingly higher temperature will be required to thoroughly melt the compound. Thus, as common rosin melts at 275°, the whole mixture must be raised to at least that temperature when it is introduced. The material may be worked at somewhat higher temperatures than those indicated without interfering with the results.

Lime, or any compound of lime, may be added to the plastic compound; and when it is used, it is not necessary to perform the operation of moulding or casting quite so rapidly as when there is no lime in it. A similar benefit is obtained by adding a little petroleum or other oil to the plastic compound, particularly when rosin is one of the ingredients. Fragrant or scent-giving ingredients may be added to the compound; and it is one of the advantages of adding a little lime, that it prevents the smell of the sulphur from being quite so easily felt.

The new material, when made with charred sawdust, is of a dark colour—almost black—and imitating ebony or similar dark wood. The addition of lime modifies the colour a little. The material will bear a high polish. It may be used with advantage for door handles, orna-

mental mouldings, beadings, and every variety of turners' and cabinet-makers' work; for ornamental clock cases, jewel and other boxes, and general decorative paraphernalia.

IV.—*On the Present State of some Branches of Iron Metallurgy.* By MR. ST. JOHN VINCENT DAY.

Read April 1, 1868.

As a mere art, solely practised according to the rough-and-ready teachings of "rules of thumb," the manufacture of iron no longer remains; for during the last decade more especially, the most skilful masters in science have applied themselves to the solution of numerous important metallurgical problems,—the necessary investigations thus given birth to having laid the foundation for building up metallurgy in that advanced state wherein it may now in many branches be certainly considered to exist, daily progressing onwards towards completion; although, from the latest discoveries that have been made, the metallurgist himself cannot avoid being deeply impressed with the conviction that he is as yet labouring only in the outer circle of that vast area which has to be travelled over, by continual plodding and perpetual discovery, ere that he can see the entire truths of metallurgic science laid out in order, each in its own place around a given centre.

The rule of thumb, the routine of the trade, the acquired habit, both the art as well as prejudice of the past time and that now passing by us, are yielding to the all-conquering subjugation of scientific discovery, mathematical calculation, and chemical analysis. At the present moment, then, we are in the midst of a vast revolution in metallurgic practice. Surrounded by the chaotic confusion and turmoil necessary upon such mighty changes, we scarcely know on which of the many processes that surround us to-day to make our stand, lest in an extremely short time something greatly superior and immeasurably more advantageous crosses our path,—to the oblivion, because inferiority, of that concerning which we had beforehand heard so much predicted, and around which our hopes and interests are rallied. And although we cannot strictly mark out which precise steps in the progress of abstract science have been the nearest causes for recent changes in practice, yet we do find that all the modern innovations group themselves around two great ideas now practically realized, each of which has

formed, and is yet extending into an important series of inventions. These now practically realized notions, which we may consider in a metaphorical sense as centres out of which the changes in modern practice radiate, are:—*first*, the use of gaseous in place of solid fuel; and *second*, the application of a forced air blast for decarburizing, desilicatizing, and in general purifying the crude iron.

Before proceeding to treat directly of the art and science of iron metallurgy, it is right that I should shortly consider what may be called the commercial state of it at the present time, and the causes which have led to the present condition in that respect; for although at first sight the introduction of this element into this paper is not strictly contemplated by its title, still, we shall find that by looking more intimately at the style of treatment which metallurgy has received in other countries, and comparing that with the more general practice at home, it is only by the more accurate paying attention to the highest teachings of applied science which has placed many countries in their present elevated metallurgical prosperity, whilst as a nation we must feel humbled in having to admit that the neglect of such teachings is the cause of much depression at home. I think none of us fail to perceive that commercial prosperity now does, and more than ever in the future will, depend upon the careful attention to what science dictates: and we shall find that the so-called "men of experience"— "the thoroughly practical men,"—and meaning by these terms those numerous heads of departments too often to be found at home, who work by mere dogma, without in any case troubling themselves to inquire as to principles, such we must find—nay, we do even yet find— giving place to the man who knows the principles and the application of them upon which his craft is based. The so-called "experienced" and "practical" man is in many cases so completely a mere rule-of-thumb-taught animal, that in reality, dependent on him alone, we have been misguided, and either have stood still or have taken such leaps in the dark, that now, when the evil is upon us, we see the cause, and the cry for "technical education" is vehement.

As to the Effecting of Economy in Fuel.

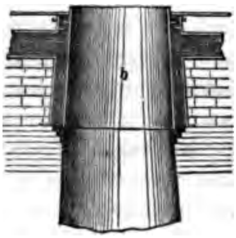
Equally important as the possession of the materials from which iron is made, is the reduction to a minimum of cost of the fuel by which these ores are reduced to the metallic state. For many years past it has been the practice in Continental ironworks to take off what is called the "waste gas" from blast furnaces, and employ them for heating the boilers which drive the blowing engines; also for heating the blast. This was practised at Wasseralfingen, in

Wurtemburg, as long ago as 1832 A. D.; * and perhaps in no other case have we a more striking example of the slowness of British manufacturing enterprise to avail itself of the means so obvious for effecting an enormous economy. At last, in a certain district in the North of England, a healthy state of matters has shown itself. The Cleveland ironmasters are not only setting a most telling example to their competitors at home, but the most refined practice in the world is conducted in that locality. To a person unacquainted with blast furnace practice, beyond being merely accustomed to see tons of useful fuel flung away into the atmosphere as so much blaze of burnt gas, the furnaces in Cleveland would, to such an one, appear not to be in operation, as there the gas is, in most cases, now all utilized—with what effects I shall show in a subsequent part of this paper. In Scotland a partial taking off of the gas was some years ago introduced at the Coltness Ironworks, for the purpose of calcining a clayband ore which is used there. This, however, is only a very partial employment of it.

As a rule, in this country we find producers are accustomed to take

* At page 469, *Metallurgy, Iron and Steel*, by Dr. Percy, he translates the following from *Der Flammenofenbetrieb mit brennbaren Gasen zu Veckerhagen*, von Hütteninspector C. Pfort, 1842; being an account of an arrangement of a furnace working with charcoal as the fuel, at Veckerhagen. It is represented in the accompanying wood engravings: Fig. 1 is a vertical section of the mouth and throat of the furnace, through the centre; and fig. 2 a horizontal section. Two cylinders of cast iron, *a* and *b*, each 5 feet high and 1 inch thick, but of which one is 1 foot wider than the other, form the mouth. The inner cylinder, *b*, is a prolongation upwards of the shaft of the furnace, and between the outside of it and the inside of the outer cylinder is an annular space, 6 inches broad, from which the gas passes through the passages, *c*, *d*, *e*, provided with dampers, so that it may be sent through any one or more at will. From *c*, the gas was used to heat the blast; from *d*, to raise steam for an engine of six horse-power; and from *e*, to carbonize wood in kilns. At Ebbw Vale the gas has been taken off in a similar manner, except that only a single cylinder, 7 feet 9 inches in diameter and 7 feet 9 inches high, was inserted in the mouth, the gas being withdrawn from the space between the outside of it and the adjacent lining of the furnace.

Fig. 1.



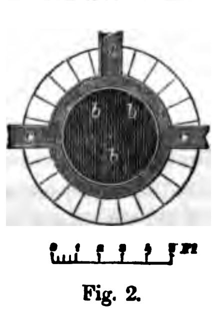


Fig. 2.

a broad aspect of a manufacture, and from that way of treating it their appreciation of details is not nearly so complete as in the case of most Continental manufacturers. From this view many work, and without troubling themselves very much whether, in their practice, they are following the most exalted application of principles, they have long remained content in making, by such and such a mode of treatment, as much profit as they can, and there have rested.

Fortunately for the iron manufacturers of Great Britain, it generally happens that the fuel is raised in combination with, or in close proximity to, the ores; so that frequently one set of mines, and their accompaniment of raising gear, is sufficient. On this very account our ironmasters have been able to make large profits, troubling themselves no further; but all the while other nations have advanced, whilst, in respect of iron metallurgy, we to too great an extent have stood still,—the effects of which we are now beginning to feel in many instances rather acutely.

It certainly is a good old proverb to "let well alone," but we must not forget to consider and act in accordance to the degree we may trust it. Unfortunately, too many of us are apt to rest our oars heavily on the assurance it brings. In no case has its real value been more strictly exemplified than in that of the British iron manufacturer, who, now that our neighbours are doing for themselves all, and even more than they formerly relied on Great Britain to do for them, begins to feel that the race he has to run is with a set of fellow-men of far higher trained intellects, and who conduct their manufacturing processes in a far more exalted and truthful way than has yet obtained, except in the Cleveland district, but in very few instances connected with iron manufacture at home. Without having attended to all the points of detail in a manner most accurate—without having sought after, and thereby discovered, means for economizing every item of cost—it had been now, as it was but a few years ago, impossible for the Continental iron manufacturer to compete successfully with the British. But what do we find? Why, this: that in countries where the fuel has to be brought for miles to the furnaces (these being usually placed near the sources of ore)—where both fuel as well as the other materials are raised from the earth, often at enormous depths, and therefore at an expenditure far higher than anything we witness at home—still, with all these disadvantages, our neighbours are now able to receive orders from this country, and execute them 5 per cent. to 20 per cent. cheaper than they can be met at home. I do not, of course, mean to say that this capacity of cheaper production is entirely due to the attention paid in every detail

of manufacture, by reducing each to its cheapest degree consistent with the highest quality of material and workmanship,—for the lower price paid for labour, and other causes which I need not enter into now, tend to effect an ultimate economy. And whilst I am fully aware that there are at the present time numerous men, some of them certainly to be considered able judges, who have and still are deprecating our present manufacturing condition to an undue degree, still it is impossible for any one who is in that position, from his own special knowledge, and who is unfettered by trade or party interests, if he be led to consider the present state of nearly every part of the European iron manufacture, and compare with it the present state of the British —all the better if he be able to form a fair opinion from previous knowledge of the state of both ten years, or even less, ago—and seeing the strides Europe has made in this branch of industry when compared to Britain, to arrive at any other conclusion than that, whereas in that time, at home, we have made no widespread advance, elsewhere works have been planted in localities a few years ago all wood and dale. All over the Continent—in Russia, as well as Siberia—such works have found their way into existence, which now stand up as such mighty opponents to us in that quiet security we formerly enjoyed. Therefore we are alarmed; and surely not without reason; for it is not possible to foresee how the state of our iron manufacture generally is ever to be so lucrative as it once was. I particularly desire it to be understood that these remarks are confined to the producers of raw material; for there can be no doubt left in the mind of a competent judge, who has had the opportunity of comparing the work of Continental nations in converting material into structural uses, such as the manufacture of engines and machinery, that in general design they are frequently inferior to the best types produced in Great Britain. At home we may certainly take credit for surpassing the rest of the world in excellence of design for moving machinery—for adapting each part so as best to fulfil its own special function —and that of course generally means the most economical, and therefore advantageous, disposition of material: in other words, our ideas as to form or proportion appear generally carried out in a much more perfect way than elsewhere. The proportions and forms of some parts of machinery made on the Continent and in America are in many cases simply ridiculous; and in general, where we find a real excellence of design in a mechanical construction, whether it be a marine or locomotive engine, or a machine tool, it is usual to discover that such has been produced under British superintendence abroad, or not unfrequently copied from British models. These views, I should add, have been

gradually formed in my mind through a long course of consideration of the subject; and they have been chiefly extended of late by the opportunities of comparison offered by a recent Continental trip, but more especially by a careful examination personally made at the recent exhibition in Paris.

Hæmatites.

But, to return to the more direct subject of this paper, it is right for me to dwell briefly upon one of the ores of iron which has of late come so largely into request on account of the enormous trade growing up as the direct result of Bessemer's great invention. The hæmatites are the only ores which can be successfully employed by the Bessemer process, on account of their comparative freedom from sulphur and phosphorus, the highly oxidizing influence of the blast injected into the convertor having entirely failed to eliminate these two substances from other ores. The hæmatite ore is an anhydrous sesquioxide of iron, and occurs in four different conditions:—

A. Amorphous, massive, and very dense, showing a conchoidal fracture, and of a bluish-gray colour.

B. In mamillated concretions known as kidney ore, which, when fractured, shows a radiated fibrous structure, of silky lustre and a steel-gray colour.

C. As a soft pulverulent mass.

D. As specular iron.

The knowledge of the existence of these ores is of very ancient date, and there is reason to believe they were worked as long ago as the time when the Romans occupied this country, and who manufactured so largely from the Forest of Dean deposits; since which time, and until quite recently, these hæmatite beds had remained almost untouched. The peculiar property of iron made from hæmatite ores is *red-shortness;* and for that reason, by itself, it is comparatively worthless for being used as a material from which to make malleable iron. Some years since, however, its admixture with certain less pure ores was found to greatly improve the iron made from them. The hæmatite deposits of West Cumberland spread over an area of about 20 square miles. Its geological position is not very definite, as in some cases there are indications of its having been deposited during the carboniferous, at others during the permian, epoch. It is usually found in large irregularly-shaped beds known as pockets, and sometimes in long sinuous veins. The deposits sometimes, although not frequently, are above 30 feet thick, but generally about 8 feet.*

* In Scotland, while hæmatites are known to exist in several localities, but

Reduction of Ores to the Metallic State.

Under this head there is very little, if anything, novel that may yet be considered worth our while to dwell upon, but I may

few of them have been yet worked, either from some defect in the ores themselves, or from the sites of their existence being awkwardly placed for transit to existing ironworks or the sources of fuel. At Whytock, in Ayrshire, a highly silicious hæmatite ore was long worked; and since then deposits have been discovered as far north as Aberdeenshire and Morayshire, and more recently in the south, near Auchencairn, Kircudbrightshire, as well as in Haddingtonshire. For the following analyses of the Aberdeenshire and Morayshire hæmatites I am indebted to Messrs. Granger Brothers of this city, who hold the minerals at Donside, Forres, and Fort Augustus:—

ANALYSES.

DONSIDE.

		I.	II.	III.
Peroxide of Iron, . .	per cent.,	65·36	81·45	76·08
Silica, ...	,,	21·34	6·08	12·36
Alumina, . . .	,,	2·32	2·14	3·20
Metallic Iron from each,	,,	45·75	57·02	53·25

FORRES.

Peroxide of Iron, ...	per cent.,	74·00
Silica, ...	,,	13·30
Alumina, ...	,,	3·66
Metallic Iron, ...	,,	51·80

FORT AUGUSTUS.

Peroxide of Iron, ...	per cent.,	74·40
Protoxide, ...	,,	6·47
Silica, ...	,,	10·72

It is singular that no sulphur or phosphorus make their appearance in these analyses. It is hardly to be supposed that none exist, as other hæmatites, although always containing these elements in less quantity than any other British ores, nevertheless do contain them in proportions easily detected. I suspect that—what is so often the case with commercial analyses—they were not carefully looked for.

AUCHENCAIRN ORE.

This has been reported upon by the well-known metallurgist, Mr. Robert Mushet; and I present the following analyses and remarks from his report:—

"THE LARGE KIDNEY ORE No. 1.—This is a red hæmatite kidney iron ore precisely similar to the hæmatite kidney iron ore of the Whitehaven district in Cumberland. It is composed as under:—

Peroxide of Iron, ...	88·00
Silica, ...	11·53
Alumina, ...	·47
	100·00 parts.

And its yield in metallic iron is 61·60 per cent.

merely mention that some attempts have been made to effect the direct reduction of the ores by gaseous fuel, among others by that very learned metallurgist Mr. C. W. Siemens. The object has been to supplant the blast furnace; but so far, I believe, there has been no real success. Indeed, in the present state of our knowledge of the series of changes which it is necessary for the ore to undergo by a properly-ordered succession of actions upon it, it is difficult to foresee how the employment of anything very far different from the blast furnace is to be brought about; for as the materials of the charge gradually descend, they are being as gradually brought under a series of influences

"No. 1 Small Kidney Ore.—This is almost perfectly pure peroxide of iron, and is composed as under:—

Peroxide of Iron, ...	98·43
Silica, ...	1·57
Alumina, a trace, ...	0·00
	100·00 parts.

The yield in metallic iron is 68·91 per cent.

"No. 2 Black Metal.—This is a compact black oxide of iron, composed as follows:—

Peroxide of Iron, ...	79·46
Silica, ...	14·07
Alumina, ...	1·36
Moisture, ...	5·11
	100·00 parts.

The yield in metallic iron is 55·63 per cent.

"No. 2 Black Metal Kidney Ore and Red Ore mixed as worked together.

"The red ore or stone in this mixture is rich in iron, ranging from 44 to 63 per cent. yield in metallic iron. Its average composition is as follows:—

Peroxide of Iron, ...	73·98
Silica, ...	17·45
Alumina, ...	1·04
Magnesia, ...	3·22
Moisture, ...	4·31
	100·00 parts.

The yield in metallic iron is 51·79 per cent.

"B No. 1 Iron Ore.—This is a nearly pure oxide of iron, composed as follows:—

Peroxide of Iron, ...	97·33
Silica and Moisture, ...	2·67
	100·00 parts.

The yield in metallic iron is 68·14 per cent."

Specimens of these various ores were exhibited to the Society, as well as others from various parts of England, including the Devonshire ores.

by which the metal and its impurities are separated, or the ore reduced, by the time it reaches the lower regions of the furnace. Mr. Siemens has put his process in operation at his own works in Birmingham, from which a small piece of steel was produced and exhibited last year at Paris. It was tried at Barrow in Furness, also at the Atlas Works, Glasgow, but in both cases, I understand, he succeeded only in making slag or very coarse glass. However, we must not speak for or against this until it is subjected to a more rigid proof. Other processes —that of Mr. Henderson's of this city, and one or two for making steel from Cleveland pigs—are said to be ripening into existence; but their being as yet in the experimental stage alone, it is prudent at present to do no more than mention them.

Then, although there is nothing new or marked as yet practically developed in the methods of reduction themselves, there have been some experiments made in this locality of late which point towards the effecting of considerable economy in smelting the richly carbonaceous ironstones. These have been made by Mr. Henry Aitken, of the Almond Ironworks, Falkirk, and consist in retaining as much as possible of the carbon in the ore by carburizing or coking it in ovens or closed pits, in place of driving and burning it off by the old mode of calcining in open air-heaps. Now, as all ironstones contain considerable quantities of carbon,—some, however, such as the blackbands and ores from the coal-measures, in much greater proportion than others,—it is evident that if we can retain the carbon, or a great portion of it, we need so much less coke or coal for reducing it in the blast furnace. I am informed that in some experiments with blackband ironstone, enough carbon has in this way been retained to effect the reduction of the ore with half the quantity of fuel hitherto used. It is difficult to see any valid reason why this mode of treating the ore is not the very right thing to do; and although the experiments are as yet in a very crude state, they are, in my opinion, so important that I have felt it a duty thus far to mention them.*

I now proceed to consider the present state of some branches that we may give more particular attention to, as being the present practice.

Blast Furnaces.

In the construction of blast furnaces there is not much to note except in the enormous increase of dimensions. In the older iron districts of this country but few new furnaces have been recently

* At the time of reading the paper the experiments were in a crude state. I learn, however, now (May 16), that one at least of the Almond furnaces is in constant work with the carburized ironstone.

erected; they therefore remain much as they have been for a very long period. The case, however, is different in the more newly developed iron-producing localities of Cleveland, North Lancashire, West Cumberland, and parts of Wales and Scotland. The present rule is in most cases an increasing height and diameter, although there is reason to fear, as I shall hereafter show, that height in some instances has been carried to too great an extent. I propose to treat of the peculiarities of blast furnace practice in each of these districts separately. Passing over Staffordshire and what we may call the Midland iron district, wherein there is nothing particular to recount that is not found in a more marked degree elsewhere, we may at once proceed to the Cleveland district.

Cleveland.

In this district the largest furnaces in the world, and the most economical modes of working, are to be found. The Acklam furnaces at Middlesborough (in vertical section, Fig. 3), built by Mr. Beckton, were three years ago the largest ever constructed, being 22½ feet in diameter at the boshes, and 70 feet high from the bottom of the hearth to the top of the brickwork. Each of these is capable of holding 1,250 tons of material, and turn out about 350 tons of iron per week. In these furnaces the angle of the boshes is much steeper than it has hitherto been the practice to adopt, being 68° with the horizon, while one of the Ormsby furnaces, belonging to Messrs. Cochrane, has its boshes inclined as much as 75°. In the construction of blast furnaces the proportion of masonry employed is, proportionately to the dimensions, much reduced. Instead of being constructed entirely of stone and brickwork, held together by iron hoops at intervals, the masonry is now frequently built up inside a malleable-iron jacket which envelops it. The direct result of these large furnaces has been the effecting of very great economy. Instead of using 36 cwt. of Durham coke per ton of pig iron produced, that is now reduced to 26 cwt.—in some cases even less—as it is said some of these produce 1 ton of pigs with 21 cwt. of coke from an argillaceous ironstone containing only about 31 per cent. of iron. Messrs. Bolckow, Vaughan, & Co. erected two furnaces, characteristic by their peculiar proportions, of only 16 feet boshes, and 95 feet high. These furnaces have worked well, and there has not been, so far as I understand, any difficulty in getting the blast to ascend through the materials, neither have the furnaces choked or "gobbed up," as was supposed; and instead of consuming 1 ton of coal per ton of pig iron as formerly, for heating boilers, blast ovens, and calcining kilns, 10 cwt. suffices. The blast is now heated to much higher temperatures.

A few years ago the maximum temperature employed was 500°, this has now crept up to 700°, 800°, 900°, and even 1,050° and 1,100°, quite recently, and the probabilities are we shall ere long employ higher temperatures still. A great part of this economy is attributable

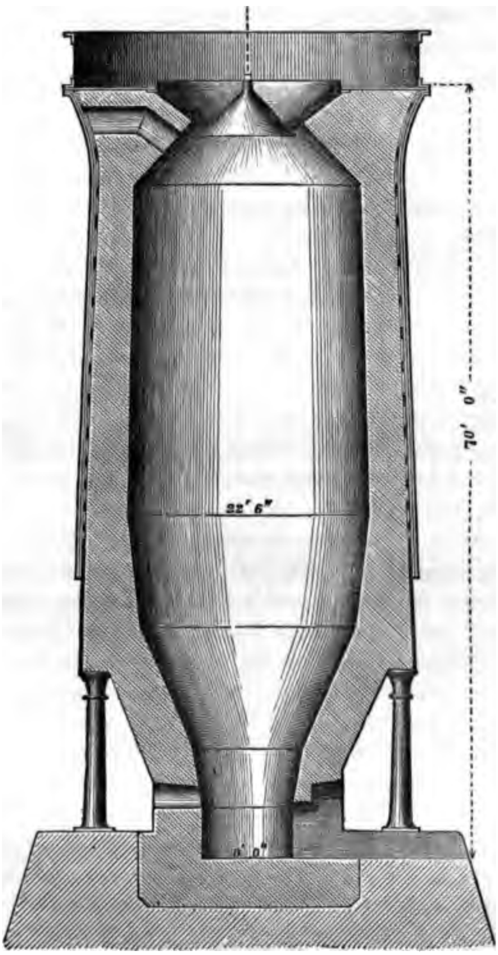


Fig. 3.

directly to the use of blast furnace gases for firing the boilers and blast ovens, and much, no doubt, to an increased efficiency in management.

The materials employed in the Acklam furnaces average about 6 tons to each ton of pig iron produced, the proportions of each being—

	TONS.
Cleveland ironstone,	3·5
Limestone,	0·75
Coke,	1·30
Coal,	0·50
	6·05

or about 4·25 tons of raw materials, independent of the fuel.

Since the erection of the Acklam as well as the Ormsby furnaces, and through their economical working having become known, dimensions have still further increased. Indeed, in the Cleveland district capacities per furnace of from 25,000 to 30,000 cubic feet are becoming the rule, the boshes of some furnaces at Stockton being as much as 26 feet in diameter; whilst the Ferryhill and Rosedale Co. have erected a pair 27 feet in diameter, with a height of as much as 102 feet. Unquestionably the ironmasters of this district have set a good example to those in Wales, Staffordshire, and Scotland; and the economy they have effected, by paying attention to true principles in construction and practice, has been seriously felt in this locality by a great stagnation in the demand for Scotch iron, owing to the lower price at which the Cleveland masters are able and willing to supply the market, whilst they, too, labour under the disadvantage of possessing an inferior ore to deal with. Fig. 4 is a vertical section of another of the furnaces of this district (Tees-side), the dimensions being fully laid down upon it. They are indeed affording a most marked lesson to Scotch ironmasters more especially; and as soon as the contemplated means for diminishing the cost of carriage of iron from the Cleveland district to the South is carried out, there can be no doubt but that, in other districts, it will be compulsory to alter the furnaces and mode of working them, to meet the increasing competition that will then present itself. As it is, so much economy is found to depend on the mere size of the furnace, that the older furnaces in the Cleveland district are, as soon as blown out, immediately enlarged, to enable the earlier to compete successfully with the more recent practice.

In considering the present state of this particular division of my subject, I should leave it incomplete did I not attempt to show some, at least, of the causes why increase of dimensions is found so conducive to economical working. Directly, it is due to the great power of burthen of the coke of the district, generally known as Durham coke. Its resistance to compression was determined a few

years ago by Mr. Bell, of the Clarence Ironworks, and he found that a 2-inch cube carried while cold a weight of 25 cwt., and when hot, 1 ton, before crushing. The ironstone is not broken up into small lumps as elsewhere, but charged in large blocks, so that, combined with

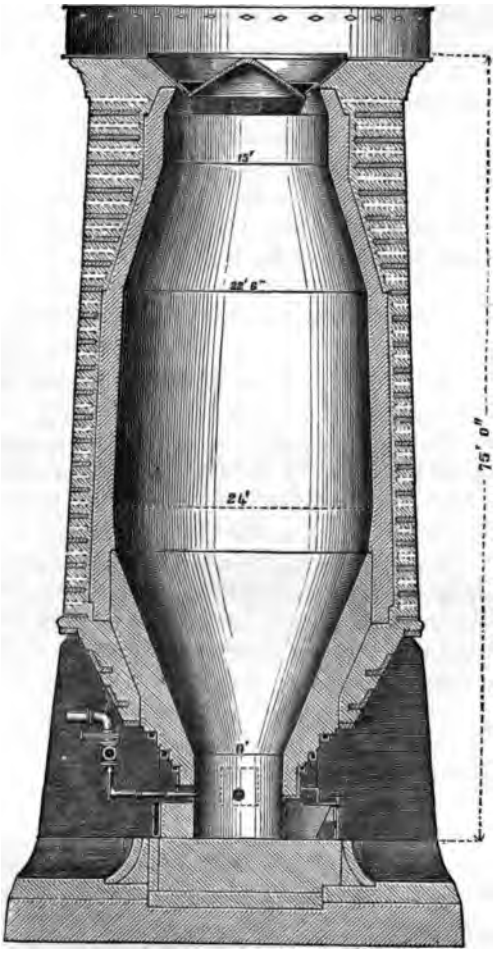


Fig. 4.

the great supporting power of the coke, the use of ironstone in this form enables a very high column of materials to be supported, whilst the space between the lumps of stone is larger, and the choking of the furnace by the weight of the charge is thus prevented, giving at the

same time plenty of freedom for the passage upwards of the blast and gases evolved from the lower regions of the charge. With the great height employed, the maximum quantity of heat which it is possible to so utilize is taken up by the charge, and therefore the temperature of the escaping gases is lower than in furnaces of a less height. These considerations point out distinctly that it would be mere waste of fuel to use a higher temperature in furnaces of less height, because the temperature of the escaping gases would be necessarily higher; and thus we see that the economical upper limit of temperature which it is possible to use is strictly dependent upon that which governs the height of the furnace—viz., the burthen which the materials in the furnace are capable of carrying—so that the more we increase the height of our furnaces, in the same ratio may we increase the temperature and pressure or column of the blast—the latter to enable it sufficient ease of ascent through the increased depth of charge.

It is very important to our iron industry to know the compressile resistance of the various kinds of coke employed in blast furnaces; and since it is evident how largely the exact knowledge and utilization to its utmost of this particular is likely to affect our pig iron manufacturers, it is to be hoped that they will not be slow to institute a set of experiments to determine it. I do not know of the existence of any series of reliable experiments on this point; but judging from the most recent practice in Wales, at the Cwm Celyn Ironworks, where a furnace of enormous capacity has been erected, the resistance to crushing of the coal or coke there employed must be far below that of the Durham coke. It is to increase of diameter also that economy is due; for if we double that, we increase the capacity fourfold by only doubling the radiating surface. There can hardly be a doubt left in the mind of any one, I think, who is capable of forming a fair estimate, but that all furnaces, even in Scotland, where we have the difficulty of a soft caking coal to contend with, but that the furnaces might be enlarged with immense advantage. Such practice, following as closely upon that of the Cleveland ironmasters as the nature of the materials will admit, would save an enormous sum to the Staffordshire, Welsh, and Scotch districts.

Barrow, in Furness.

At Barrow, in Furness, the furnaces are arranged in groups of six—two of which groups are, I believe, now complete, those last built being the larger. The geographical position of these works, which are close upon the sea-shore, constituting one feature of the material facilities the Company possess for exporting the produce, together with the

command of boundless capital, as well as the scientific treatment by which the manufacture is governed, place these works in one of the most commanding positions in the world. The blast furnaces are not so large as those in the Cleveland district, the largest being 56 feet high and 16 feet maximum inner diameter. The tapping takes place every six hours, giving about 20 tons at each "run out." One ton of coke is used per ton of iron produced,—this being remarkably low when compared with the consumption in some other districts. But, of course, much of this economy depends upon the rich quality of the hæmatite ore smelted, which contains from 55 to 60 per cent. of iron; but as the silicon is also present in large per centage, as much as 30 per cent. of lime is required to flux it.

The charge per ton of iron produced runs thus:—

	Cwt.
Raw ironstone, ...	34
Coke from Darlington, from . .	19 to 21
Limestone, ...	6·5 to 7

At these furnaces part only of the "waste gases" are taken off—this being effected on a plan introduced by Mr. J. J. Smith, a manager. Each furnace has six openings at the crown, separated from each other by brickwork. These form a ring at the centre, which constitutes the support for a vertical gas tube of brick. The openings serve for charging the furnace through, and as the materials are maintained at a level considerably above the crown, the greater portion of the gases pass off by the central tube. Those portions of gas which do escape are burnt in a flame at the furnace top.* Each furnace has six tuyeres, from

* The furnace has a dome at its top formed of six arches of fire-bricks, *c*, Fig. 5, which unite at the crown in a ring of 4 feet internal diameter, *d*, and thereby form the basis for the vertical gas tube erected upon the top of the ring. The vertical brick cylinder which forms the gas tube is surrounded by an outer cylinder, *d*, constructed of fire-bricks, and resting upon the inner brickwork of the furnace itself. This is 11 feet 6 inches internal diameter, and has six side openings, *l*, opposite to the holes formed between the six arches of the dome. These side openings serve as filling holes for charging the furnace, and the materials are kept at a level considerably above the crown of the dome, so that the greater portion of the gases is drawn off through the central tube, and only the remainder makes its escape up through the filling holes and between the two vertical cylinders.

Mr. J. T. Smith has also applied another arrangement, dispensing with the internal vertical tube, and carrying the gases off sideways instead of taking them to the top of the outer cylinder first. In both forms the arrangement has given satisfaction on account of its certainty, and its permanent action without the necessity for frequent repairs.

2·5 to 3·5 inches diameter; the blast pressure from 3 to 3·5 lbs. per square inch; and is heated from 600° to 650° Fahrenheit.

In charging these furnaces, it is the practice to throw in the materials through five of the openings in succession, and not to charge

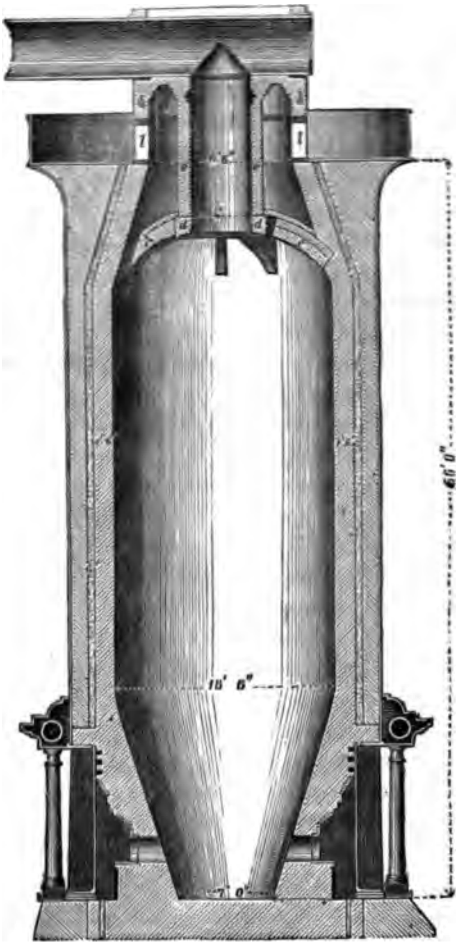


Fig. 5.

through the sixth—the object being to produce a somewhat spiral layer of loose materials next the sides of the furnace; and it is found that the sides are thus maintained in better condition, whilst any irregularity in the gathering of the materials in any one place is prevented. After one charging round is completed, except through the last open-

ing, the same operation is repeated, care being taken to commence each rotation through the next opening in front of that through which the last preceding rotation had been commenced.

Scotch Blast Furnaces.

To a large extent in Scotland the masters have the good fortune of possessing a rich ironstone in contact with the fuel necessary for smelting it, whilst the furnaces are placed almost at the pit's mouth, and in these respects the Scotch ironmasters stand in a better position than any others. It is, no doubt, owing to first costs being thereby so much lower than in other districts that our producers have troubled themselves so little about economical working. The waste of fuel in Scotch blast furnaces is most notorious; and while, even in these dull times, when most of them are blown out, a most fitting opportunity to make alterations or improvements, it is a deploring truth to utter, that we do not find the ironmasters using any means for placing themselves in that position whereby they may, if they will, compete more successfully than of late with the Cleveland masters, and enable themselves to hold that particular, and to a great extent supreme, position which they so have long enjoyed. But we may rely that the time is not far distant when, if they intend to hold their own, they must submit to and follow the teachings of science in practice. Let us briefly consider what the present state of blast furnace practice in Scotland is.

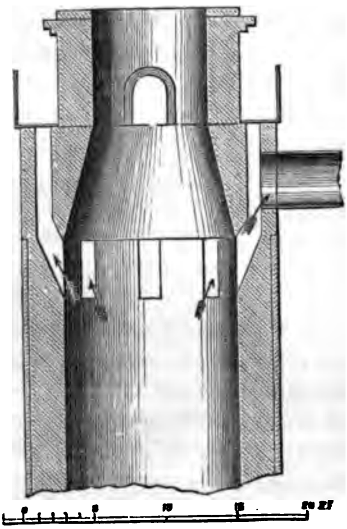


Fig. 6.*

We possess a richly carbonaceous ore, which, when calcined, contains from 55 to 70 per cent. of metallic iron. It is reduced at a lower temperature than many other ironstones; and, containing 0·5 to 0·75 per cent. of phosphorus, although certainly less than some other ironstones contain, it melts at a low temperature. The fuel used for its reduction in the blast furnace is raw coal. Now, from 2 tons to (in some

* Fig. 6 shows the top of one of the Dundyvan furnaces, and the arrangements for taking off the gases therefrom a few years since practised.

cases above) 2·5 tons of coal are charged into the furnace per ton of pig iron produced, and this is quite independent of that employed in addition for heating boilers for driving blowing engines and heating blast, as we must not forget that not an inch of gas escaping from the furnace is utilized, but the whole of it recklessly permitted to cast its heat-power into the surrounding atmosphere; whereas in using coal for smelting, the volume of gases which escapes is immensely greater than with coke, as all the volatile substances pass off in this way, and are therefore available as so much more fuel capable of being usefully applied in the works. But, not to be hasty, let us compare this state of matters with that of Cumberland and Cleveland. In the former case, when previously speaking of the practice at Barrow, it was shown that from 19 to 21 cwt. of coke was used per ton of iron produced; and in Cleveland as low as 17 cwt. is sometimes obtained. Now, to make a just comparison with Scotch practice, we must convert the quantity of coal employed into its equivalent coke, because it is only those parts in it which would form coke if it were carburized that even when used as coal are really available for smelting. Scotch coal contains from about 57 per cent. to 65 per cent. of matter which will form coke; and if we suppose, as we safely may, 60 per cent. for an average, we find that, in coke value, about 30 cwt. is used per ton of pigs produced; that is to say, in other words, that in Scotland, including the fuel for blast ovens and boilers, and working with the richest ore, the fuel consumed per ton of iron is more than double that of the best English practice with the most inferior ores to deal with. Neither may we omit to consider, in addition, that whereas in Scotland only from 2 to 2·25 tons of ore and limestone are charged per ton of iron, at Cleveland 4·25 tons of ore and limestone are used; while in the hæmatite district, where the per centage of iron in the ore is not much below that in the Scotch blackbands, the weight of the two materials is about 2 tons.

The foregoing are not all the facts that may be shown up as indicative of the evils going on at our very door; for we must consider the production per furnace. In Scotland this is seldom over 200 tons per week in the largest furnaces—more frequently less—whilst at Barrow this reaches 500 tons, and even more, per week. What then, under these facts, is it the duty of our ironmasters to do? Most assuredly not to live in *laissez faire* any longer. The remedy is easy. In the first place, let them utilize their "waste gases;" and surely as it is found in the Cleveland district to pay to pull down their old furnaces and build larger, the same thing must be done in Scotland. It is true the soft coal here prevents the carrying of great burthen, hence it is no use to increase the height of the furnaces, but we may enlarge

their bulk; for, as I have before shown, although we quadruple the capacity of the furnaces by increasing the diameter, we only double the radiating surface.* I am aware that exception may be taken to pulling down existing furnaces, on account of the necessity for immense immediate outlay. Then, although the furnaces remain as they are, that is no reason why the gases from Scotch blast furnaces should not be utilized, for at present certainly less than one-half of the fuel is all that is effectively employed. To show that it will not do to increase the height of Scotch furnaces it is only necessary to mention that, with their present height of 40 to 50 feet, it is difficult, when the materials approach the tuyeres, to keep the hearth clear of solid matter falling down, this necessitating the employment of several men to keep the hearth clear from such lumps, which they lift out through the cinder-hole by means of bars. As to increasing the temperature or pressure of the blast, it will be no use to do either, for the furnaces are not high enough to give any advantage by so doing.

Welsh Blast Furnaces.

In the present blast furnace practice of Wales there is little to recount but what is treated of in the preceding remarks as applied to other localities. I may, however, allude to some of the Ebbw Vale Co.'s furnaces.

At Shirhowey, in Tredegar, the Ebbw Vale Iron Company have four furnaces, the highest of which is 72 feet 3 inches, and 16 feet diameter

* I believe that, at the Coltness Ironworks, Mr. Houldsworth many years ago utilized, with notable economy, some of the waste gases for calcining a clayband ironstone which is smelted there; and I hear that recently, at Langloan, Messrs. Addie have been conducting some experiments for utilizing the gases, according to a plan proposed by Mr. F. Kohn; but I am not yet sufficiently acquainted with the details to state what results have been obtained. The accompanying fig. 7. Near the top, an annular flue or channel, *d*, is formed in the thickness of the sides, and this annular channel has a ring of rectangular or other shaped apertures, *e*, forming the communication between it and the interior of the furnace. On the opposite sides of the furnace, two openings are made in the wall, on the outside of the annular flue or channel, *d*, for the insertion of two elbow branches opening into two pipes or flues, *f*. The opposite ends of these pipes terminate or open into the main pipe or flue, *g*, which communicates by branch pipes, *h*, placed upon it at convenient distances, with the calcining or roasting ovens, *i*. Each kiln has two inlet pipes for the gas. Each branch, *h*, in place of passing directly into the kiln, opens into a small detached furnace or fire-place, *j*, placed there for the purpose of igniting the gases previous to their entering the kiln by the pipes, *k*, opening from the upper part of the ignition furnace into the annular flue or channel, *l*, formed in the thickness of the wall, near the base of the kiln.

at the boshes. Its make is 250 tons per week; and it is chiefly employed in smelting the white and brown spathose ores from the Brendon hills in Somersetshire. It makes a good iron for conversion into steel by the Bessemer convertor. The fuel is coked, whilst at other works in Wales it is used raw.

In some of the Welsh ironworks the cost of fuel has recently been much economized by the introduction of coal-washing. The small coal formerly was lost; but this is now separated by washing from pyrites, shale, and other impurities, which, from their higher specific gravity, sink to the bottom of hopper-shaped tanks, which are kept full of water. The bottoms are opened at intervals, and the heavier portions then fall out, the remaining material being afterwards coked, making a fuel very free from sulphur. Coal-washing has long been practised on the Continent; and the sooner we see its universal introduction into Wales and Scotland, and, indeed, into every place where coal is used in the blast furnace, the better.

We find generally in Europe that the trade of the older iron districts is on the decline. We are, indeed, passing from "an age of iron" to "an age of steel"—this being one of the results that have attended the introduction of the Bessemer process. Wherever we go we meet with blast furnaces being erected for smelting the red hæmatite ores—the only ore yet available for producing iron that can be successfully used in the convertor, on account of its small per centage of sulphur and phosphorus. Nearly all the blast furnaces that have been erected in this country during the last five years are in the hæmatite districts of Lancashire and Cumberland. In Germany the demand for this material is so great that new furnaces are springing up in the hæmatite districts there also. In Hanover the Georgs Marienhütte has increased its number of blast furnaces to five, whilst another great iron-producing district is springing up near the Saar, where there is fortunately abundance of coal that makes a good coke. The ore, which is argillaceous, lies on the surface of the ground, and contains about 33 per cent. of iron. The French and Prussian governments have carried out the canalization of the river to Saarbrücken, and new blast furnaces are being, or about to be, erected on its banks. From the immense facilities here possessed in every way for producing iron at a most economical rate, and considering also its geographical position, there can be little doubt but it will become one of the most important iron-producing centres of Europe. In Prussia the Cöln Musen Com-

pany have erected five or six new blast furnaces for the production of spiegeleisen pigs; and in Siegen, the very centre of the spiegeleisen manufacture, the production is continually increasing; and in many of the ironworks where charcoal was formerly employed, this has been substituted by coke, adding considerably to the production of the furnaces—the fuel being brought from the neighbourhood of Essen by the railway which has only recently been opened up. In France the iron manufacture is in a less fortunate state than elsewhere on the Continent; for, possessing usually very poor ores at home, they have to import from abroad. England, Elba, Sardinia, and Algiers, are the sources of ore from which the best iron in France is obtained.

Blast Stoves.

With the high temperature of blast employed in the Cleveland district, some persons thought the heating pipes would be rapidly deteriorated. Now, however, it is the practice to provide a much larger extent of surface per volume of blast forced through them. More than a foot of surface is now allowed per cubic foot of blast supplied per minute; whilst with temperature near 300° Fahrenheit, as formerly employed, from 250 to 500 feet of surface only were provided per 1,000 feet of blast per minute. That is now raised in the Cleveland district to between 1,200 and 1,300.

Conversion of Pig Iron into Malleable Iron and Steel.

In the conversion of pig iron into malleable iron, this is generally effected on the long-practised method ascribed to Cort; but the rapid growth of newer modes point unmistakeably to the discontinuance of that practice, and the adoption of the true and more rational means which have so recently presented themselves. Cheap fuel is a commodity that every malleable iron manufacturer may now avail himself of. He need be no longer dependent upon having the fuel stores in the immediate locality of his works, neither need he feel so much concern for the cost of transit from a long distance, as the employment of gaseous fuel enables the utilization of the very commonest and worthless dross, and even lignites that we may chance to obtain.

In the early part of this paper I mentioned as a source of the great progress of present metallurgic practice the employment of gaseous fuel. This, most of us know, is very largely carried out by Mr. C. W. Siemens. For metallurgic purposes its value seems to have

been first appreciated by M. Mayr of Leoben, in Styria, who was the earliest to use it for making crucible steel. The Styrian ironworks are situated unfavourably with respect of fuel supply; and as the Siemens regenerative furnace is capable of utilizing the most inferior kinds of materials, M. Mayr was by its means enabled to use lignite, which, when burnt in the ordinary manner, will not produce enough heat for metallurgic purposes.

In Great Britain, as well as all over the Continent, the Siemens furnace is now largely used in iron and steel works for puddling and melting iron and steel and reheating blooms. In France its use has paved the way for one, if not two, highly valuable processes, which without it could not have been worked. I refer to the Martin steel process and that of M. Bérard—the former an established method of working now in other countries besides France, and the latter not yet fully developed. There can be little doubt that, as soon as circumstances permit, we shall find the Siemens furnace—or some modification of it, as Mr. Gorman's of this city, and which is, I believe, in use at the Govan Ironworks—exclusively used for puddling, for by such arrangements we are enabled to alter the character of the flame to any state we may require according to the nature of the material we desire at any time to produce in the furnace.

Richardson Process.

I have now to lay before you some remarks connected with certain departures from the ordinary run of malleable iron manufacture, which have been developed chiefly in this locality, but now practised at two or three works in England, and recently put in operation at Sireuile, in France. At the same time, preparations are being made for its extension into Belgium, Austria, and elsewhere. What I refer to is that which has become known as the "Richardson process." So far as that mode of working has yet been employed (for the whole of it has not been fully put into operation), it consists in injecting a blast of compressed air below the surface of a charge of molten pig iron in the puddling furnace immediately that the iron is melted, and continuing to inject it until the metal begins to boil, at which time the injection is stopped—in other words, the blast is withdrawn. The effect of introducing a "forced air blast" at this particular stage is to decarburize and desilicatize the iron in a much more rapid way than by the mere passing of oxygen over the charge, as hitherto practised; and instead of occupying from twenty-five to thirty-five minutes to bring the iron to what is known as "the boil," this by the forced air

blast is effected in from ten to fifteen minutes. In this manner it is evident that at this stage of conversion great economy of time is obtained; and the other impurities, besides carbon and silicon, are oxidized out of the iron in a much more thorough degree than has been hitherto obtained. After the injection of blast is stopped, the charge is worked up in the ordinary manner of puddled iron. As far as the time occupied in puddling a charge is concerned, this, with common iron, is reduced from a quarter of an hour upwards. With a better class of pig iron the economy of time is greater still. Now the direct effect of the higher purifying influence thus brought to act upon the charge is to enable inferior pig iron to be used for making a better finished iron than has yet been obtained from the best pigs. The iron is considerably stronger; indeed, some of the bars made at Parkhead, which have been tested at Govan, give, according to Mr. Beardmore's statement, a tensile strength of 27 tons per square inch—that is, I dare say most of the members of this society are aware, 5 tons above the Admiralty standard. Then, again, the extreme certainty of the process is remarkable to a degree. Out of the hundreds of tons of iron that have now been made in this manner, not a single bad bar has been met with; whereas we know that after having been in use for nearly a whole century, even now puddling cannot be relied upon to produce continuously a similar quality of metal.

The period from the melting to the boil with the iron used at Parkhead, under the common method of puddling, occupies from twenty-five to forty minutes; this, by the Richardson process, is reduced to ten minutes. The temperature of the furnace being higher, the period from the commencement of the boil to that when decarburization and desilicatization are completed, and the iron separates from the slag and cinder, is again reduced. But the balling operation is of a little longer duration than under the old method, on account also of the greater temperature, the granules of iron requiring time to cool down to that temperature at which they adhere when pressed together.

In these experiments the time of working a charge composed entirely of Scotch pig was brought down to one hour and eight minutes, whereas the usual time is from one hour thirty minutes to one hour forty-five minutes under the old method. The yield is considerably higher; so high, indeed, as to require only 21 cwt. 1 qr. 17 lb. of pig iron to produce a ton of malleable iron, showing a loss on the conversion of only about six per cent. Then as to the purity of the iron, it is remarkable. Two samples which I sent to Dr. Stevenson Macadam, of Edinburgh, to be analyzed, have given the following results:—

ANALYSIS.

NAME OF ELEMENT.	SQUARE BAR.	ROUND BAR.
Iron,	99·569	99·648
Carbon,	0·035	0·031
Silicon,	0·076	0·075
Sulphur,	0·025	0·028
Phosphorus,	0·031	0·034
Manganese,	trace	trace
	99·736	99·816

We see the sulphur and phosphorus are within the merest shadow of being entirely eliminated, evidently showing that some special influence must be present, when puddling iron according to Richardson's system, that is not found elsewhere. Iron never has been made from British ores on a commercial scale so pure as in these two instances. Made from inferior Scotch pig, which always contains such a very large per centage of these two elements, the extent of their elimination is all the more remarkable.

We have now to consider the extent to which phosphorus and sulphur are removed in the ordinary process of puddling. On the present occasion, then, I shall, to shorten the time I am occupying, merely bring forward analyses from the very best makes of malleable iron I have met with, some of these being taken from Dr. Percy's excellent work *On Iron and Steel.* An armour plate made at Lowmoor was found to contain 0·104 per cent. of sulphur and 0·106 of phosphorus. Now, the mean values for Yorkshire pig iron are—

Sulphur,	0·052
Phosphorus,	0·549

in which case, either in the puddling or re-heating furnace, but most probably in the former, the sulphur is doubled, but the phosphorus diminished by the very great amount of ·433 per cent. This, however, being an armour plate, is a special case, and it is highly probable that a portion of better pig iron was mixed with the Yorkshire pigs ordinarily used at Lowmoor. The sulphur is considerably increased: it may possibly have been added from the fuel. In several other specimens of armour plates we find sulphur existing in the following percentages:—

Sulphur, 0·058, 0·121, 0·190, 0·118, 0·058, 0·104, and phosphorus in the following:—

Phosphorus, 0·030, 0·173, 0·020, 0·228, 0·089, 0·106.

In regard to sulphur, it is higher in the examined plates than in all the different qualities of British pig iron I have met with except two—namely, Northamptonshire and Scotch; and since it is not in the least likely that either of these pigs were used in producing the plates, we may infer that sulphur has been added to the iron during its con-

version and working into the malleable state, whilst phosphorus is eliminated in every case to a very large extent, bringing its percentage considerably lower than in any known analysis of British pig iron. Comparing again malleable iron of South Staffordshire with the pig, we have for the former,—

Sulphur, ...	0·165
Phosphorus, ...	0·140

Here also the sulphur is increased and the phosphorus diminished to a very considerable extent, and the same result obtains generally with any other comparisons we may make. We are therefore led to infer that during the conversion of pig into malleable iron, sulphur is added to the iron from the fuel, whilst phosphorus is eliminated in a very large ratio. Of course, in considering the increase that takes place, we must bear in mind that the per centage in malleable iron is greater in proportion to the absolute amount of iron itself present than obtains in the pig iron; for the reason, that in the latter case the carbon and silicon are present, whereas in the former they are nearly eliminated.

Comparing the foregoing results with analyses of iron made by the "Richardson process," the superior eliminating power of that process is forcibly striking. In the analysed samples of the iron made at the Glasgow Ironworks by that method, as I have before stated, for the experiments inferior brands of Scotch pigs were used; and yet from such material we have obtained finished bar iron purer than anything ever heretofore manufactured from British ores; and that, too, from pig irons which contain a medium percentage of sulphur, and considerably above a medium of phosphorus: indeed, the removal of these two elements is so nearly complete that practically they are not found. There can be no doubt remaining that the Richardson process has effected what has never before been done in Great Britain. By it we are enabled to produce a better and purer iron from the commonest pigs than we can yet depend on obtaining continuously by the common process of puddling from the best pigs. We can save a third of the time hitherto occupied in puddling, get a higher yield, save fuel, and therefore cheapen the production of malleable iron.

YIELDS WITH OLD METHOD OF PUDDLING.

Thursday, 19th March, 1868.	*cwt.*	*qr.*	*lb.*
No. 13 Double Furnace, ...	44	2	10
— 15 Do., ...	44	1	25
— 29 Single Furnace, ...	21	3	20
— 17 Do., ...	21	1	10
Friday, 20th March.			
No. 13 Double Furnace, ...	42	2	25
— 29 Single Furnace, ...	21	2	25
Wednesday, 25th March.			
No. 13 Double Furnace, ...	44	2	10

YIELDS WITH NEW METHOD OF PUDDLING.

Thursday, 19th March, 1868.	*cwt.*	*qr.*	*lb.*
No. 1 Double Furnace, ...	45	1	15
— 4 Do., ...	45	2	15
— 3 Single Furnace, ...	22	0	5
— 18 Do., ...	21	3	5
Friday, 20th March.			
No. 4 Double Furnace, ...	45	2	0
— 18 Single Furnace, ...	22	1	15
Wednesday, 25th March.			
No. 1 Double Furnace, ...	45	0	0

Here we see at a glance that in every case the yield is higher by the new than by the old method.

For the last five months there have been several furnaces making iron by this process at Parkhead, and the mode of working is being extended to all the furnaces there. I have a large number of the returns as to the time of puddling a charge, besides a large number of results which I have noted myself from watching the conduct of the process on several occasions. These, however, I have not had sufficient time to tabulate; but I may state that the time from the moment of charging to taking out the balls varies from one hour fifteen minutes to one hour thirty minutes; whilst in another experiment that we made, having previously "fettled" the furnace very heavily all round, both the bridge sides and flue, the heat was out in one hour and twenty minutes. This, without the fettling, would correspond to about one hour and ten minutes—a remarkably short time when we remember that at this forge, where very grey pig iron is principally used, the amount of time usually occupied for puddling by the old method is one hour and forty-five minutes, and very often about two hours. The period of melting in the case of such pig iron is very high. I have on several occasions noted it, and find it to vary between thirty-five and fifty minutes. This is doubtless due in a great measure to the large proportion of silicon contained in it, as well as the hæmatite, a small portion of which enters into the mixture. Out of the hundreds of bars that have now been made in this manner at Parkhead, from the "common mixtures" too, it is particularly important to remark that not a single bad bar has been met with. The iron works better under the shingling hammer as well as the rolls. In the puddled bars, after they have passed through the mill and are cooled, their appearance externally is frequently as smooth and even as good finished merchant bars, whilst the fracture shows the perfect regularity of the metal. With puddled bars of the same mixture, made in the ordinary manner, when cold and worked to the same extent, they are full of flaws and fissures gaping wide open at the surface, and the fractures always show much want of uniformity, while the microscope reveals the presence of a great deal of graphitic carbon.

It has occurred to me that I should direct attention to the now well-ascertained fact, that in the puddling furnace some of the phosphorus passes from the charge into the cinder, but by no means the whole of it. This circumstance led Dr. Percy to propound his theory, that still more of that element, existing in the charge as phosphide of iron, was liquated or sweated out after the metal was balled. If there be any truth in this supposition, it appears to be confirmed by the results of the mode of working to which I have now called your attention.

The balling process is of longer duration, as I have already explained; therefore greater time is afforded for the supposed liquidation of the phosphide. I merely throw out this as a hint; it is rather soon yet to attempt to form a conclusive theory. That phosphorus is not got rid of by oxidation in combination with iron is certain, or it would unquestionably be eliminated in the Bessemer convertor, every means hitherto tried having utterly failed to remove it.

Puddled bars from the common mixtures will not usually bend through a greater angle than 40° without cracking right across; some of them break off quite *cold-short* before having reached that angle; whilst the best bars of such mixtures rarely if ever bend 45°, and usually break in half before reaching that extent of flexure; but with bars made by the "Richardson process," out of precisely the same materials, I have seen them, I may with certainty say on most occasions, bend completely round to 90° without breaking, frequently without showing a surface crack, and some specimens that have been tried were doubled up without effecting a complete fracture. Altogether, the advantages of working by this process are so great as to require no further comment from me.

I need not refer in more than a passing mention to Bessemer's great invention, as I take it for granted that the members of this Society are well versed in it; but I do wish to point out a particular field for chemical investigation, which holds out to the discoverer a reward far beyond anything we can at present estimate. What I allude to is the finding out of a cheap and therefore practical method of dephosphorizing and desulphurizing pig iron. Purely on this account Bessemer is confined to the employment of hæmatite iron, as all the other ores and pig irons contain this element to such an extent that the metal made from them is worthless both on account of *cold-shortness* as well as *red-shortness*.

V.—*On the Sources of Sulphur used in the Manufacture of Oil of Vitriol, or Sulphuric Acid.* By MR. JAMES MACTEAR.

Read in the Chemical Section, April 29, 1868.

THIS paper is intended as a short sketch or description of the sources, properties, and comparative values of brimstone (as imported from Sicily), and the various qualities of iron and copper pyrites now or formerly used in the manufacture of oil of vitriol, or sulphuric acid.

In the first place, it may be as well to state that the quantity of oil of vitriol annually manufactured in this country alone exceeds 500,000 tons, of which quantity somewhere about 320,000 tons are used in the conversion of common salt into sulphate of soda, for the production of alkali, by Le Blanc's famous process,—a process, by the way, which, invented by an obscure apothecary about the year 1794, has as yet been able to hold its own against all others which the advancement of chemical science has enabled inventors to produce, and yet repaid its originator by allowing him to die in a workhouse.

The manufacture of soda was introduced into this country in the year 1802 by Mr. Lash, at his works at Walker, near Newcastle: and from this period almost every year has shown an increase in the quantity of alkali manufactured, and consequently in the sulphur consumed, until during the past year it has reached the large amount of 160,000 tons, of which, however, only from 10,000 to 20,000 tons was in the form of brimstone; the remainder was in the shape of pyrites, of which upwards of 375,000 tons have been burned.

As stated, about 320,000 tons of the acid produced were used in the alkali manufacture, the remainder being taken up chiefly in the manufacture of artificial manures, of which a large quantity is now made and used in putting new life into the soil, which we of the "enlightened nineteenth century" so systematically rob of its natural right by converting our streams and rivers into common sewers, and carefully getting rid, by means of their channels, of that best of all manures of which we hear so much under the titles of "town sewage," &c., and bringing down on our heads the wrath of the famous Baron Liebig.

It will be unnecessary to say more regarding brimstone as a source of oil of vitriol than that it is chiefly used in the manufacture of sale acid. This is mostly used for bleaching purposes, which require a very pure acid, and when that made from pyrites would not answer, owing to its being always more or less contaminated with arsenic and various matters, which, although very small in quantity, are certainly there, and now and then prove their presence by damaging some article in the preparation of which it is being used.

But as regards pyrites the case is different: the qualities are so numerous, and vary so much in their burning properties, that a good knowledge of their peculiarities is of great use to our manufacturing chemists. Until the year 1840 there is no mention made of the use of pyrites as a source of sulphur, the whole of the sulphur used up to that time having been obtained, in the shape of brimstone, from Sicily. But in that year the price rose from about £6, 10*s*. to £13 per ton, owing to the fact that the king of Sicily had granted to a Marseilles

firm a monopoly of the export of sulphur. These gentlemen, "hasting to become rich," compelled the manufacturers of Britain to look for some cheaper source of supply; which they were not long in obtaining in the various ores of pyrites or bisulphide of iron. It has been stated, however, that previous to this time a Mr. Hill of Deptford had, in 1818, used this material in the manufacture of oil of vitriol; but this, though probable, does not seem sufficiently authenticated to permit of our fixing that early date for the introduction of pyrites as a source of sulphur to the manufacturing industry of the country.

The monopoly was very soon abolished, and the price of brimstone reduced; but many of the manufacturers still continued using the pyrites in their works. Since the year 1851 brimstone has usually been high in price, chiefly owing to the great demand for sulphur in the vine districts for the treatment of the much-dreaded vine disease.

The first pyrites would seem to have been brought from Cornwall and Ireland, with the exception of a small quantity of "coal brasses" —a bisulphide of iron found in the coal-fields in some quantity; and up to the year 1856 no other ores were used. But in that year some cargoes of Spanish pyrites, containing small quantities of copper, were imported to the Tyne; and so well was this quality liked by the manufacturers, that now at least one-half of the ore used is Spanish cupriferous pyrites. One of the chief reasons for the preference of the Spanish pyrites is that it contains a high percentage of sulphur; and as there is the same cost per ton in working pyrites of a low strength as there is with that of a high percentage, it follows that by using the latter there is more work done for a given sum. Irish pyrites contains about 35 per cent. of sulphur, while Spanish is about 45 to 50 per cent.; and supposing that the burning costs for labour 2*s.* per ton, there is in the one case 35 parts of sulphur burned off for the 2*s.*, and in the other 45 parts. Besides this, it is found that the refuse, or burned pyrites, contains as much sulphur in the one case as in the other—on the average about 5 per cent.—so that there is actually burned off, in the case of Irish pyrites, 86 per cent., and in the case of Spanish (45 per cent.) 89 per cent. of the sulphur in the ore—a clear gain of 3 per cent.

There are a number of disadvantages connected with the employment of iron pyrites instead of Sicilian sulphur, which must be taken into consideration. These are—

First.—In burning pyrites there is required a considerably greater amount of chamber space than with sulphur. This is usually taken to be in the proportion of 45 to 30. Owing to the fact that the iron in the pyrites is converted into the peroxide during the combustion, there

is required a much larger quantity of air than in the case of brimstone; and as the nitrogen from the air passes through the burner or kiln unchanged, we have to provide extra space for it in the chambers. That this is a disadvantage will be seen at once on remembering that when bisulphide of iron is burned we get 2 atoms of sulphurous acid and half an atom of peroxide of iron ($2\ Fe\ S_2 + O = 4\ S\ O_2 + Fe_2\ O_3$). Or, in other words, for every 32 parts of sulphur burned off we have 12 parts of oxygen replacing it in combination with the iron. This we may call extra oxygen, as it does not play any part in the chambers; but the nitrogen which had been mixed with it in the form of air (which is composed of 77 parts (by weight) of nitrogen and 23 of oxygen), equal to 31 parts, passes into the chambers, where it is of "no manner of use," and only monopolizes space.

Second.—The arrangements for burning are much more costly; and as the heat is greater, there is more tear and wear of the chambers and connecting pipes. There is also a destructive action set up on the lead by the sublimation of various metals from the glowing mass of pyrites.

I have recently been informed that a German manufacturer has tried an alloy of lead and antimony for building his chambers, but have not yet heard how it lasts.

Third.—There is always a larger percentage of nitrate of soda used when burning pyrites.

Fourth.—Owing to various causes the production of acid is not so good.

Fifth.—It involves more labour in working, and also yields a refuse approximately three-fourths the weight of the raw ore, and for which deposit room has to be provided.

Sixth.—There is produced in burning and during transit a considerable amount of small, which cannot properly be burned in the kilns, and requires a separate furnace.

The most common is a muffle furnace, where the smalls are roasted, with free admission of air, the great quantity of which passing into the chambers in proportion to the sulphur, is the chief objection to this method.

But taking all these into account, when ordinary sulphuric acid is manufactured, it is found that one ton of sulphur in form of pyrites costs only about £4 10*s*., while Sicilian sulphur costs £6 to £7. The pyrites is usually sold at so much per cent. of sulphur per ton. For some years the rate was 1*s*. per unit, and at present it is about 8*d*.

It may be as well to state here that the yield of oil of vitriol ($H_2\ S\ O_4$) from 100 parts of pure sulphur is theoretically 306·25 parts, and

in practice, using brimstone of say one per cent. impurity, the actual yield has been obtained of 302 per cent. With pyrites the production is rarely above 285 per cent. of the sulphur actually burned, or about 275 per cent. on that in the original pyrites.

I have here a number of specimens of brimstone and pyrites, and first,

Sicilian Sulphur.

In the volcanic districts of Sicily, but chiefly on the south coast, large masses of sulphur are found, in a more or less pure state, imbedded in lime and clay marl. It is usually accepted that the sulphur has been formed by the double decomposition of hydro-sulphuric and sulphurous acid gases ascending through the cracks and crevices of the rocks. But Bischof has shown that if hydro-sulphuric acid (or sulphuretted hydrogen) gas be passed through carbonate of lime at a temperature of 212° Fahr., the chalk is converted into gypsum, while sulphur is deposited. As in most cases both chalk and gypsum are found along with the brimstone, there can be little doubt that this decomposition does take place to some extent, and a considerable part of the sulphur has been deposited in this way.

The sulphur is extracted from the crude mineral in various ways, depending on the more or less richness of the material. If very rich, it is simply melted in a cast-iron pot, allowed to settle, and ladled into moulds. If, on the contrary, it be poor in sulphur—some of it only containing 15 to 20 per cent.—it is distilled in earthen retorts, and condensed, usually in water.

As imported into this country, it consists of three qualities, technically known as firsts, seconds, and thirds. With the two former we have little to do, as they are chiefly refined for the manufacture of gunpowder, &c., while the last is used in the manufacture of oil of vitriol.

The apparatus used for this purpose, or rather the part of it in which the sulphurous acid is produced, is comparatively simple, being an iron plate surrounded and arched over by brickwork. Under the plate is a flue, to allow of a circulation of air, in order to prevent the heat rising so high as to sublime any of the sulphur. The charge of brimstone is thrown in by the door on to the plate, and allowed to burn off at as low a heat as possible.

As already stated, the consumption of brimstone for this purpose has decreased very materially; and we now chiefly depend for our supply of sulphur on the various qualities of pyrites, of which I will now speak:—

Spanish Pyrites.

This claims the first attention, owing to its now forming at least one-half of all the pyrites burnt in the manufacture of sulphuric acid. It is found chiefly in the province of Huelva, the deposits, however, not being confined to this part of the country, but extending into Portugal. It exists in large masses, some of them nearly a mile long, of great depth and of varying thickness.

Until very recently the great bulk of the pyrites was imported from the mines of Mr. Mason, which are situate near the river Guadiana. This gentleman was the first to construct a railway as a means of transport to the shipping port, instead of the old-fashioned and expensive mule carriage.

In all other mines mules are still employed, the pyrites being carried in sacks or baskets on their backs. But a few years since a company, composed to a great extent of Glasgow gentlemen, purchased a number of the mines, and now work them under the name of the "Tharsis Mining Company." The pyrites from these mines is now largely used, and have reduced the imports of Mason's ore very considerably.

The amount of sulphur in these pyrites varies. Mason's specimen contains nearly 50 per cent., with about 3 or 4 per cent. of copper. The Tharsis pyrites contains 48 to 50 per cent. of sulphur, and 4 to 5 per cent. of copper.

I have here a specimen of a pyrites from Spain, but have been unable to obtain the name of the mine. It contains only 36 per cent. of sulphur and about 1 per cent. of copper, and is a very inferior ore in all respects.

These pyrites, except the last named, are much the same in their working qualities,—burning well, and making comparatively little dust or small in breaking. The first cargoes of the Tharsis, however, contained a good deal of a soft black ore, containing a large quantity of copper, but which crumbles down on breaking, making a great deal of small. Very little of this quality is now met with.

Norwegian Pyrites.

There are a number of mines in Norway, of which the chief are situate near Drontheim. The pyrites from these mines have been pretty largely imported into this country, chiefly to the Tyne.

The largest quantity is from the mines of Ytteröen, where from 6,000 to 8,000 tons are raised yearly. This pyrites is seen to be composed of very small crystals, and is of very good quality—burning well,

and not slagging in the kilns. It contains about 44 per cent. of sulphur and from 1 to 2 per cent. of copper.

There is a second quality of this pyrites imported in some quantity. It is formed of the dressings of the best pyrites, and contains sometimes considerable quantities of carbonate of lime and silica. Both of these are very objectionable ingredients, as the lime always retains an equivalent of sulphur (getting converted into sulphate), and the silica is apt to fuse and envelop the particles of pyrites, thereby preventing the proper expulsion of the sulphur. The residue from this quality has always a test of about 2 per cent. above that of the first quality.

A small quantity of pyrites from a mine about thirty miles from Drontheim is also imported, and is much liked by manufacturers, as it contains no copper (or at most but a trace), and is hard and dense, resembling very much the pyrites from Spain. It burns off well, and gives a residue with a low test of sulphur.

Not long ago some cargoes of pyrites were imported from a mine which had been opened near Bergen. It is of good quality, and burns well. It contains about 46 per cent. of sulphur.

Another quality of Norwegian pyrites comes from Nordland, containing 42 per cent. of sulphur, and burning well in the kilns.

Swedish Pyrites.

The Swedish pyrites is obtained in mining for copper ores, and is said to exist in enormous quantities; but at present the cost of transport to a shipping port is too great to allow of its being exported to any extent. A few cargoes, however, do find their way into this country, and are found to work very well. The cargo from which this specimen was taken tested 40 to 44 per cent. of sulphur. It made a good deal of small in breaking.

Belgian Pyrites.

This variety is imported very largely into this country, the greater part being to the river Tyne, the freights from Antwerp, where it is shipped, being very cheap—sometimes only sixpence per ton.

The mines are situate in the districts of Liege and Namur, and are worked more especially for the ores of zinc and lead, the pyrites being obtained during the operation. It is very peculiar in its appearance. One kind is called alluvial pyrites, and has the form of coprolites. Few of the pieces being much over a pound in weight, it cannot well be burned alone; but if mixed in small proportion with Norwegian or other ores it can be burned to advantage.

Another variety has the appearance of a slag or cinder, and often contains antimony and lead. It burns pretty well, but is apt to get too hot in the kilns.

At most of the mines the pyrites is picked and sifted; and there are now large accumulations of small or dust pyrites, of which very little can be sold. The Belgian ores contain from 40 to 50 per cent. of sulphur, and some traces of thullium.

Westphalian Pyrites.

This pyrites is rather peculiar—looking almost like a very poor fire-clay or shale; but it burns very well, and is not liable to flux in the kiln. It is said to contain thullium in considerable quantity. It contains from 42 to 45 per cent. of sulphur.

Italian Pyrites.

Only a small quantity of this pyrites has been imported, as it is found to be so brittle as to make a large proportion of smalls in transit and in breaking for the kilns. It is remarkable in its appearance, being composed of well-defined crystals, loosely cemented together, probably by silica, of which it contains 9 or 10 per cent.

It contains about 45 per cent. of sulphur, and burns badly, falling to powder and running into slag in the kilns.

Irish Pyrites.

A very large quantity of pyrites was at one time obtained from the Wicklow mines, where it exists in beds of great thickness. It contains only 30 to 35 per cent. of sulphur, is a very poor burning ore, and little used at the present time. There is a deposit in the Vale of Ovoca containing about 44 per cent. of sulphur; but of this last I have had no experience, as but little of it has been imported.

Cornish Pyrites.

Under this head I include the pyrites ore from the mining districts of Devon, Dorset, and Cornwall. It is obtained in dressing the copper and lead ores for market, and is often in small pieces; it contains a good deal of arsenic; and at present little of it is consumed. It contains 25 to 30 per cent. of sulphur and 1 or 2 per cent. of copper.

Mr. Vivian, of Swansea, is now, I believe, manufacturing sulphuric acid from copper pyrites, burning off the sulphur in a kiln of peculiar construction.

Coal Brasses.

A considerable quantity of this article, sometimes called also "Scotch

gold," is annually obtained from the coal districts; and if well cleaned, it is a *cheap* source of sulphur for the manufacture of acid for manure makers, &c.

It contains, even when well cleaned, a considerable quantity of organic matter, which is much objected to by most manufacturers, as, besides colouring the vitriol brown, it is found that the products of its decomposition, proceeding into the chambers, necessitate the use of a larger quantity of nitrate of soda than is usually required. The sulphur in brasses generally costs from 2*d.* to 4*d.* per unit. Brasses often contain carbonate of lime, each equivalent of which renders unavailable a corresponding quantity of sulphur.

With all its faults, however, coal brasses are sometimes useful in keeping up the heat of the kilns when using some of the other varieties of pyrites, which are apt to be cold in burning.

Cleveland Pyrites.

This variety, although interesting as a deposit to the geologist, deserves but little notice as a source of sulphur, as it contains only about 25 per cent., and is only used at one establishment at Cargo Fleet, near Middlesbro' on Tees. It is a very poor ore, and requires great care to burn it properly.

TABLE OF ANALYSES OF PYRITES.

CONSTITUENTS.	Spanish Pyrites. "Mason's."	Spanish Pyrites. "Tharsis."	Norwegian First Quality.	Norwegian Non-cuprious.	Swedish Pyrites.	Belgian Pyrites.	West-phalian Pyrites.	Italian Pyrites.
Sulphur,	49·60	49·30	44·50	50·60	43·50	45·01	45·60	47·00
Iron,	42·88	41·41	39·22	44·62	38·90	39·68	38·52	42·20
Copper,	2·26	5·82	1·80	......	·50	......	......	......
Lead,	·50	·66	......	......	·20	·37	·64	trace
Zinc,	·20	trace	1·18	1·40	2·65	1·80	6·00	......
Arsenic,	·28	·31	......	......	·10	......	trace	trace
Thallium,	trace	......	......	......	......	trace	trace	......
Lime,	·20	·10	3·60	......	1·00	·25	·11	·21
Magnesia,	trace	trace	trace	......	·56	......	......	·60
Oxygen as Oxide of Iron,	·15	·25	·45	......	·30	·32	·37	·24
Insoluble Matter and Moisture,	3·99	2·05	9·25	3·50	12·25	12·48	9·06	9·37
	100·06	99·90	100·00	100·12	99·96	99·91	100·30	99·62

Having given a description of the great sources of the sulphur supply, I have only to notice one which, although, properly speaking, is not a *source*, will yet have some effect in reducing the quantity at present imported. I allude to the sulphur from alkali waste. As is well known, all the sulphur used in the alkali manufactures is at present lost, none of it entering into the composition of the carbonate or caustic soda, but remaining, combined with lime, in the "waste." The problem of

extracting this sulphur, and again using it in the acid manufacture, is one which has attracted the attention of most of our celebrated chemists; and Mr. Ludwig Mond, in a paper read before the chemical section of this Society, has given a very good description of the results of their efforts.

Sulphur from this source is now manufactured to a small extent, and probably, ere many years elapse, it will become a regular article of consumption.

VI.—*Notes on the Post-tertiary Geology of Norway.* By Rev. Henry W. Crosskey, F.G.S., and Mr. David Robertson.

Read April 29, 1868.

The object of these notes is to record the observations we were enabled to make, during a journey in Norway, upon the magnificent post-tertiary beds of that country, with especial reference to their curious and important relationships to the post-tertiary beds of Scotland. We have long maintained that the Scottish beds cannot be loosely classed together under any one general name. No single term can express their diversities of composition and their differences in age and fossil contents. The glacial beds represent an epoch, with fluctuations alike in climate and the relative position of land and sea. Every period in that epoch has left its mark more or less distinctly; and the proper classification of these periods is the work which now tasks the skill of the post-tertiary geologist. The study of the Norwegian beds unfolds a remarkable series of parallelisms with those of Scotland. The various stages in the history of the earth's crust during the glacial epoch, and from the glacial epoch to the present day, were (so far as our observations extended) of the same general character in Scotland and Norway, and have left corresponding signs of their nature and progress.

Before entering upon the detail of our observations, we are bound to express our obligations to the English Consul at Christiania, Prof. Sars, Dr. Boek, Hr. Hömann, and other Norwegian gentlemen, by whom we were so kindly assisted. Nothing can exceed the courtesy with which the eminent Professors in the University of Christiania direct the traveller who wishes to carry out any special study.

Landing at Christiania, we examined the series of beds in its immediate neighbourhood, and various islands in the fjord. We then

proceeded to Skien, on the north-west of Christiania fjord, and entered the Thelmarken district, visiting the Gousta mountain (6,000 feet) and the waterfall of the Rjukanfos (900 feet in height), returning to Christiania by Kongsberg and Drammen. In the course of this route we were able to examine typical beds belonging to various parts of the Norwegian series.

I. The lowest boulder clay, wherever we observed it (and it is scattered over large districts), is precisely analogous to the lowest boulder clay of Scotland.

(1.) Its position is identical. It occurs in those long, undulating waves, so familiar in any section over Clydesdale—evidently the result of the hollowing action of water after its deposition. It is cut through by rivers, as in a fine section at Honefos; or flanks a valley with ridges; or forms isolated hillocks; or fills up hollows—exactly as in Scotland.

(2.) Its general character is the same. It contains blocks of the same sizes, and having the same relationship to the district. The stones are striated, the mass is compact, and without stratification. There is the usual intermixture of travelled boulders, with stones from the adjacent strata.

(3.) It rests upon the grooved and polished rocks. At Ovre Foss clay pit, near Christiania, for example, a part of the boulder clay had been removed, exposing the striated surface. The grooving and polishing were so similar to the grooving and polishing on the Scotch rocks, that we cannot resist the conclusion, that in both cases a force of similar intensity was at work.

(4.) This oldest boulder clay is not the fossiliferous clay of these districts we examined, in the same way as it is not the fossiliferous clay of the west of Scotland.

We do not deny the existence of a fossiliferous boulder clay, but simply assert the existence of large masses of unfossiliferous boulder clay underlying the arctic shell beds of the glacial epoch. There is more than one clay which, through its peculiar characteristics, may be called "boulder clay." Masses of "boulder clay" may have been carried down to the sea by glaciers, or dropped from icebergs, and contain therefore fragments of shells, as in the Caithness beds. On the west of Scotland, however, the boulder clay (as far as has yet been seen) is not fossiliferous, and underlies the shell beds. It is remarkable that the great shell beds on the west of Norway do not occur in a material corresponding to our boulder clay, and that the "*boulder clay*" developed in the districts in which they occur is not fossiliferous.

This substance, which we have described as the "oldest boulder clay," is evidently that which Prof. Kjerulf speaks of as "*glacial*

banks and débris heaps," forming the lowest part of the "*older and strictly glacial group.*"*

II. At the Lower Foss clay bank, near Christiania, beneath the richly fossiliferous clay we observed a bed, curiously corresponding to the laminated mud occupying the same position in the west of Scotland. In Scotland this bed easily splits into leaves, and is rather a mud than a clay, its nature being determined by the rocks in its immediate neighbourhood. In an article "On the Glacial Deposits of the Clyde,"† it was suggested that the finely laminated clay which immediately succeeds the boulder clay was probably the deposit of rapid waters issuing from beneath ice, and carrying the excessively comminuted mud well-known in rivers issuing from Alpine districts.

This laminated clay has been described as unfossiliferous; but we have proved its marine or estuarine origin by the discovery of *Nonionina striato punctata*, although we have never detected in it any species of mollusc.

The laminated clay which we observed near Christiania corresponds evidently with that of the Clyde district. Its mineral composition is slightly different, because of the different character of the local material from which it was derived. Prof. Kjerulf describes a "marly clay," as the second of the series of "older and strictly glacial group," in which a "leafy or stratified structure may often be seen;" and gives the same account of its origin that had been given in the paper referred to, for the laminated clay of the Clyde district. He attributes its deposition to the "mud-laden waters which poured from the inland ice;" and ascribes the rarity of the shells found "in the marly clay" to "the ice-cold fresh glacier water from which it was deposited being unfavourable to molluscan life."

It is very remarkable that the analogies between the Scotch and Norwegian beds should extend to the lamination of the clay immediately underlying the rich shell clay of either district; while the same explanation of its origin has been given by observers in both countries. There are, of course, local differences. In Norway clay and sand appear to alternate; but the fact of the deposition of very fine mud, quietly and calmly, by ice-cold water unfavourable to marine life, over an area which subsequently became fit for the abode of a highly abundant arctic fauna, remains the same, and is a curious instance of the correspondence between the physical histories of Scotland and Norway.

* See Kjerulf "On Scratched Rock Surfaces," translated by Watson, *Edin. New Phil. Journal*, vol. xviii., p. 7.

† See *Trans. Geol. Soc. of Glasgow*, Part I., p. 51, Art. "On the Glacial Deposits of the Clyde," by Rev. H.W. Crosskey.

III. Next in order occur the arctic shell beds, with a fauna indicative of extreme cold.

Verlebugten, near Moss. Height 10 to 20 feet above sea level.

This bed is on the south side of Christiania fjord; and is remarkable for having the same characteristic shell as that which occurs in the clay near Errol, in the Carse of Gowrie.

Leda arctica is very large and abundant at Moss. It occurs *in situ*, and with both valves united.

Leda arctica is also extremely abundant in the clay near Errol. It is associated at Errol with the same shells, among others, as at Moss—viz., Tellina calcarea (proxima); Saxicava arctica; and Buccinum Grœnlandicum.

Errol is one of the spots at which we have the most intensely arctic fauna yet found in Scotland, and the correspondence of its characteristic shell (Leda arctica) with that of Moss is a connecting link, revealing another striking similarity between the physical conditions of Norway and Scotland during the glacial epoch.

Leda arctica is not now found living either in Christiania fjord or the Hebrides, and indicates a degree of cold equivalent to that in Greenland and Spitzbergen.

A fossil valve of Leda arctica has been dredged by Mr. Jeffreys off the west coast; and we have seen one specimen from a glacial sand in Ayrshire; but while thus scarce in the west, it is in the east an abundant fossil. This fact probably shows the same kind of difference between the east and west of Scotland during the glacial period as at the present day.

There is a difference attributable to local conditions between the clay at Moss and Errol. At Errol few stones occur, and the clay washes almost entirely away through fine muslin, leaving only a minimum residue of sand. At Moss the clay is homogeneous, without obvious stratification, and with stones interspersed of all sizes from small pebbles to blocks of between 2 and 3 feet in diameter. Some of the enclosed stones are angular and subangular; some striated; others more or less rounded and polished—precisely of the character that might have been dropped upon the sea bottom by floating ice.

The shells in the clay did not occur in zones, but were scattered through it in all parts.

Four and a half lbs. of clay, washed through a sieve of ninety-six meshes to the inch,* left a residue of 1 lb., composed of sand and gravel.

* In washing all the clays alluded to in this paper, the same sized sieve was used.

Upper and Lower Foss, near Christiania. Height, 50 to 70 feet above sea level.

(1.) This bed of shell clay rests in a hollow of the older boulder clay which rises rapidly to the surface. Its position is precisely similar to that of the Paisley glacial clays.

(2.) The Silurian rock beneath the lowest clay (which in Scotland we should call a boulder clay) is beautifully polished, grooved, and striated.

(3.) Resting against this boulder clay is a marl clay (derived from the wearing down of the Silurian strata), occurring in horizontal layers, with thin deposits of sand, and having a decided tendency to lamination. The shells are sparsely scattered through this lower marl clay.

(4.) The upper part consists of a purer clay. At Paisley we have noted the same fact. The laminated bed is unfit for brickmaking purposes; and from that upwards, the clay is always the purer. It appears to be the same at the Upper and Lower Foss. The uppermost clay of all had been worked away for bricks, according to the practice in our own neighbourhood.

(5.) At Paisley the shells occur the most abundantly after the impure clay, with its laminations, has given place to the purer clay, but again diminish in the uppermost brick clay.

As far as we could observe and learn from conversation with Prof. Sars, this had been the case at Upper and Lower Foss.

The fact is of importance in Scotch geology. It indicates the passing away of the cold water issuing from ice-fed rivers, the formation of a sea bottom from the washed boulder clay, and its gradual re-elevation, until the deep sea became the estuary, and the estuary the river,—the river itself at last narrowing to its present channel.

Nine lbs. of clay, taken from the south side of the river Aker, left 1 lb. 3 oz. residue after washing; 3 lbs. 10 oz. from the south side left a residue of 3½ oz.

We collected the following species, in addition to those given in the lists of Prof. Sars:—*

* It will be noticed that we do not give a list of all the species we collected, but in every case our lists indicate those *not previously obtained* from the beds described, and which have therefore to be added to those given by Sars in his monograph*—a monograph which is by far the ablest and most complete yet published on the subject, and to which we owe large obligations. We have to express our thanks to Mr. J. G. Jeffreys, who has examined the mollusca of our collections, and given to our lists the value of his authority, and to Mr. H. B. Brady for similar service with respect to the Foraminifera.

* *Om de i Norge Fore Kommende Fossile Dyrelevninger fra Quartærperioden et bidrag til vor Faunas historie.*

ENTOMOSTRACA.

Cythere Dunelmensis, *Norman.*
,, concinna, *Jones.*
Cytheridea punctillata, *Brady.*
,, papillosa, *Bosquet.*
,, Sorbyana.
Ilyobates Bartonensis, *Jones.*
Sclerochilus contortus, *Sars.*
Cytheropteron Montrosiensis, *new species.*

FORAMINIFERA.*

Lagena distoma, *P. & J.*
,, lævis, *Montagu.*
Dentalina communis, *D'Orb.*
Polystomella crispa, *Linn.*
,, striato-punctata, *F. & M.*
Truncatulina lobatula, *W. & J.*
Virgulina Schriebersii, *Czjzek.*†
Polymorphina lactea, *W. & J.*
Polymorphina compressa, *D'Orb.*
Biloculina elongata, *D'Orb.*
Cristellaria rotulata, *Lamk.*
Nonionina turgida, *Will.*
Lituola Canariensis, *D'Orb.*
Cassidulina lævigata, *D'Orb.*
Triloculina tricarinata, *D'Orb.*
Quinqueloculina seminulum, *Linn.*
Dentalina pauperata, *P. & J.*
,, communis, *D'Orb.*
Lagena gracillima, *Seguenza.*
,, lævis, *Montagu.*
,, distoma, *P. & J.*
Nonionina turgida, *Will.*
Bulimina marginata, *D'Orb.*
,, aculeata, *D'Orb.*
,, ovata, *D'Orb.*

Brynd. Height above sea level 240 feet.

This bed, a few miles from Christiania, belongs also to the older glacial formation. A deep bank is exposed, cut through by a small rivulet; and there is evidence of its having suffered considerable denudation. The clay rests upon the native rock of the district, and no section of any series of beds is exposed. It is very friable, and has a few stones scattered through it.

Nine lbs., when washed, left only ¼ oz. of residue, composed of fine gray sand and small fragments of quartz, gravel, schist, with broken shells.

Eight lbs. more of the clay, taken from a spot very near the first sample, left 1 oz. of residue. Although this is nearly four times more than the first, the difference does not arise from a greater proportion of sand and gravel, but from clay, fragmentary in appearance, which does not dissolve in water, like that in which it is imbedded, but remains with sharp and angular corners.

Clusters of Leda pygmæa are found in the clay, which forms a kind of nodule round them.

We collected the following species, in addition to those given in the lists of Prof. Sars:—

Mytilus edulis, . .	frgs.
Cardium echinatum, .	1 you.
,, fasciatum, . .	6 ,,
,, exiguum, . .	frag.
Thracia papyracea, *var.* villosiuscula, . .	1 you.
Scrobicularia nitida, .	frag.
,, alba, . .	,,

* We are greatly indebted to Mr. H. B. Brady for his valuable examination of all the Foraminifera. The value of the lists depends on his high authority.

† One of the specimens having the terminal chamber double and two apertures.

Psammobia Ferroënsis, . frag.
Lima Loscombii, . . „
Trophon clathratus, . . „
Odostomia unidentata, . 1
„ turrita, . . 1
„ interstincta, . 1
Utriculus truncatulus, . 1
Balanus crenatus, . . frag.
Ray spines and dermal tubercles of star-fish.
Ophiura—ray plate.
Echinus Dröbachiensis, . spines.

ENTOMOSTRACA.

Cythere lutea, *Müller.*
„ concinna, *Jones.*
„ Dunelmensis, *Norman.*
Cytheridea Sorbyana.
„ papillosa, *Bosquet.*
„ punctillata, *Brady.*
Cytheropteron latissimum, *Norman.*

FORAMINIFERA.

Lagena lævis, *Montagu.*
„ distoma, *P. & J.*
Dentalina communis, *D'Orb.*
„ consobrina, *D'Orb.*
Nonionina depressula, *W. & J.*
„ scapha, *F. & M.*
Polystomella striato-punctato, *F. & M.*
Truncatulina refulgens, *Montfort.*
Bulimina marginata, *D'Orb.*
„ aculeata, *D'Orb.*
Quinqueloculina seminulum, *Linn.*
Biloculina elongata, *D'Orb.*
Cristellaria rotulata, *Lamarck.*
Cassidulina lævigata, *D'Orb.*
„ oblonga, *Reuss.*
Bulimina pupoides, *D'Orb.*
Lituola scorpiurus, *Montfort.*

The beds now noticed at Moss, Upper and Lower Foss, and Brynd, are examples of the oldest arctic shell clays of Norway. The shells generally speaking are *in situ.*

They correspond with the beds at Errol, Elie, Paisley, and Dalmuir, in Scotland, both (1.) in their position relatively to the oldest boulder clay; (2.) their general character; and (3.) the nature of the imbedded fauna.

Prof. Sars gives sixty-one species of mollusca as the result of his examination of twenty distinct beds of this period; and of these sixty-one species we have already collected forty-eight from the oldest arctic shell clays of Scotland, and have no doubt we shall be able to add others, every fresh excavation uniformly enabling us to add new species. This result is most remarkable. The fact that only thirteen out of sixty-one species of the mollusca found in those glacial beds of Norway, indicative of the extremest cold, and extending from Trondhjem to Aremark, should be absent from the glacial beds of Scotland, indicates a close and curious correspondence of physical conditions.

The fauna as a whole constitutes, as in Scotland, a well-developed group. The condition of the shells is so nearly the same, that it would often be impossible to distinguish the Scotch from the Norwegian specimens.

We were not able to examine any of the shell, sand, and gravel banks, of littoral origin (as distinguished from the shell clays), belonging to this epoch; but their description precisely coincides with the nature of the sand and gravel bank cut through by the railway

near Drymen station. Every sea has its sand and gravel banks, as well as mud or clay deposits, and these occur in Scotland as in Norway. In both countries, the various beds indicate differences of depth and circumstances of exposure.

IV. The glacial shell beds (examples of which have now been noticed) are succeeded in Norway and Scotland by another great series of shell beds, which may be roughly grouped as "post-glacial."

They sometimes occur in the form of banks, in which shells and fragments of shells predominated over the sand and gravel—so that "shell banks" is a better descriptive name than "sand and gravel beds"—and sometimes in the form of clay beds. The condition of the various beds indicates, indeed, the condition of the different parts of an ordinary sea bottom, as determined by local circumstances.

We will proceed to notice a few typical examples which we had the opportunity of examining.

Ommedalsstrand, near Skien. Height, about 100 *feet above sea level.*

This is a magnificent terrace, nearly 40 feet high, almost entirely composed of shells and shell fragments. It is situated at the south end of the beautiful lake, Nordsaeen, and is divided into distinct masses by the course of a small river. Over an area of several acres, huge masses of this 40-feet shell bank occur. The shells, however, are generally in single valves, and often broken, although some examples are found with both valves united. Sars supposes, from the large extent of the bank, that it must belong to the Laminarian zone.

The lowest part of the deposit, resting upon the native rock of the district, is a sandy clay, but by far the larger part consists of pure sand.

The shelly débris of which the banks consist appears to have been washed up over the clay; and from the number and somewhat medley character of the shells, they were probably accumulated layer after layer.

That the bank is a wash is still more evident from the almost entire absence of clay; 7½ lbs. lost only 1½ lbs. in washing, leaving 6 lbs. residue, chiefly shell fragments, a few pebbles, and a little sand.

The shattered and broken character of many of the shells shows that they have been subjected to some violence, but their generally unpolished surface proves that they have not been long exposed to attrition.

In comparison with the number of shells, the ostracoda and foraminifera are few, and there is an unusual preponderance of individuals over species.

Some of the ostracoda, as well as a few of the shells, have both valves united. Upon several valves of shells fragments of shells and echini are strongly agglutinated.

External plates of Ophiura are found, but none of the small ray spines or internal plates; both of these, being lighter than the external plates, were probably floated away during the washing up of the beds.

Burrows of Siphunculus are to be seen in the mouths of some of the small Rissoa.

We also discovered plates of a new species of Echinus, characteristic of the glacial shell bed in the isle of Cumbrae; and for which we propose the name of Echinus Cumbraensis. We have submitted it to Prof. Sars, to whom it is quite unknown. It will be figured and described in the *Proceedings of the Geological Society of Glasgow.*

The fauna of this bed is peculiar. It contains some arctic species. Pecten islandicus is rare and small; Tellina calcarea (proxima) large and abundant. Sars records Terebratula cranium, Panopæa Norwegica, Leda pernula, Crenella discors.

The whole character of the fauna, however, is not so intensely arctic as in the series of beds previously described. There is a large increase in the number of species, and many of them denote a post-glacial climate.

It appears to us, therefore, (1.) that the bed is essentially post-glacial in character; (2.) that some peculiarly arctic species lingered in it before becoming extinct; (3.) while some arctic specimens were possibly washed into it from neighbouring arctic clays, the uppermost part of which is slightly shown in the sand clay at the base of the shell banks.

From the Ommedalsstrand and other kindred deposits it appears that the change of climate was not sudden, but that gradual amelioration took place, possibly with fluctuations to and fro.

We collected the following species, in addition to those given in the lists of Prof. Sars:—

Tapes pullastra.	
Lucina borealis.	
Trochus Grœnlandicus,	. 10
,, helicinus, .	. 2 frag.
Lepeta cœca, . .	. 7
Spirialis retroversus,	. 1
Rissoa inconspicua, .	. 4
,, globosa, .	. 1
Homalogyra atomus,	. 8
Odostomia albella, .	. 1
,, turrita, .	. 1
,, insculpta,	. 1

Cerithium perversum,	. 2
,, metula, .	. 1
Trophon clathratus, *var.* Gunneri, . . .	. 3
Trophon Barvicensis,	. 1
Fusus gracilis, . .	. 3 frag.
Utriculus obtusus, .	. 1 ,,
,, hyalina, .	. 3 ,,
Eulima bilineata, .	. 2
Lacuna divaricata, *var.* quadrifasciata, .	. 6
Scissurella crispata, .	. 1

Aclis supra-nitida,	1
Ophiocoma rosula,	plates.
Ophiura,	,,
Spatangus purpureus,	spines.

ENTOMOSTRACA.

Cythere tuberculata, *Sars.*
,, Finmarchica, *Sars.*
,, angulata, *Sars.*
,, pellucida, *Baird.*
,, villosa, *Sars.*
,, borealis, *Brady.*
Loxoconcha tamarindus, *Jones.*
Cytherura undata, *Sars.*
Xestoleberis depressa, *Sars.*

FORAMINIFERA.

Polymorphina lactea, *W. & J.*
,, compressa, *D'Orb.*
Quinqueloculina seminulum, *Linn.*
,, agglutinans, (?) *D'Orb.*
Triloculina trigonula, *Lamk.*
Lagena melo, *D'Orb.* (?)
Planorbulina Ungeriana, *D'Orb.* (?)
Cristellaria rotulata, *Lamk.* (?)
Truncatulina lobatula, *W. & J.*
Polystomella striato-punctata, *F. & M.*
Nonionina asterizans, *F. & M.*
,, depressula, *W. & J.*

N.B.—This is an obscure lot—the shells much worn, and characters in many cases effaced.

Sparebakken, by Skien.

This deposit occurs on a bank of the river connecting Nordsaeen with the sea; and is of the same age as Ommedalsstrand, although denoting a different sea bottom. Mya truncata, which in the former occurs only in broken valves, is here *in situ*, precisely as in the Clyde beds. The bed consists of a sandy clay of a grayish colour.

Eight lbs. lost 2¼ lbs. in washing, leaving a residue of 5¾ lbs., consisting of sand, waterworn stones, and shell-fragments.

The washing was rendered much more difficult by the amount of fibrous portion of mytilus found floating amongst the clay, like paper pulp. It is interesting to note how this disintegration takes place in one locality, and not in another. We have observed it in a post-glacial bed near Luss, Loch Lomond, exactly as at Sparebakken. May not one deposit contain a solvent of the tissues of the shell, and this be absent from another? If this be the fact, it would go far to explain the absence of fossils from one locality, while they are abundant possibly in its immediate neighbourhood.

While there is a sandy clay at the base, with Mya truncata *in situ*, the upper part of the bed is more sandy, and the shells are more broken and confused.

The fauna presents the same general characteristics as at Ommedalsstrand. We collected the following species, in addition to those given in the lists of Prof. Sars:—

Astarte triangularis,	1 frag.
,, borealis,	1 ,,
Scrobicularia prismatica,	frag.
Pecten islandicus,	frags.
Chiton cinereus,	7 plat.
,, ruber,	1 plat.
Lepeta cœca,	1
Trochus millegranus,	1 frag.
Rissoa parva,	8
,, ,, *var.* interrupta,	16
,, soluta,	10
Aclis unica,	1

Skenea planorbis, .	. 2
Homalogyra atomus,	. 7
Odostomia insculpta,	. 1
„ interstinata,	. 2
Eulima distorta, .	. 1
„ bilineata, .	. 1
Natica alderi, *var.* subovalis,	5
„ affinis, . .	. 2
„ Grœnlandica,	. 4
Utriculus hyalinus, .	. 2
Spirialis retroversa, .	. 1

ENTOMOSTRACA.

Cythere viridis, *Müller.*
„ pellucida, *Baird.*
„ cuneiformis, *Brang.*
„ concinna, *Jones.*
„ Finmarchica, *Sars.*
„ Dunelmensis, *Norman.*
Cytheridea punctillata, *Brady.*
„ papillosa, *Bosquet.*
„ Sorbyana.
„ dentata, *Sars.*
Loxoconcha tamarindus, *Jones.*
Xestoleberis depressa, *Sars.*
Cytherura nigrescens, *Baird.*
„ undata, *Sars.*
„ striata, *Sars.*
„ acuticosta, *Sars.*
„ gibba, *Müller.*
Sclerochilus contortus, *Sars.*
Paradoxostoma variabile, *Baird.*
Cytheropteron latissimum, *Norman.*
„ punctatum, *Brady.*

FORAMINIFERA.

Polymorphina lactea, *W. & J.*
„ compressa, *D'Orb.*
Quinqueloculina secans, *D'Orb.*
„ bicornis, *W. & J.*
„ seminulum, *Linn.*
„ subrotunda, *Montagu.*
Cornuspira foliacea (?), *Phil.*
Truncatulina lobatula, *W. & J.*
Dentalina communis, *D'Orb.*
Polymorphina horrida, *Rss.*
Nonionina scapha, *F. & M.*
Lagena distoma, *W. & J.*
„ lævis, *Montagu.*
„ striata, *Montagu.*
„ sulcata, *W. & J.*
Nodosaria pyrula, *D'Orb.*
Dentalium brevis, *D'Orb.*
Nonionina depressula, *W. & J.*
Polystomella striato-punctata, *F. & M.*
Rotalia Beccarii, *Linn.*
Planorbulina Mediterranensis, *D'Orb.*
Bulimina marginata, *D'Orb.*
Textularia sagittula, *Defrance.*
Nonionina turgida, *Will.*
Biloculina elongata, *D'Orb.*

Aafos, near Skien.

In the centre of a wood not far from Sparebakken is an isolated heap of shelly débris from 8 to 10 feet in height. It rises abruptly on the one side, and gently slopes down to pasture ground on the other. The shells are greatly broken, but do not seem waterworn in any marked degree. They are, however, greatly corroded: 8 lbs. of the débris, when washed, left 6 lbs. of residue, which consisted of shelly matter and sand. The plates of Balani were abundant.

The disintegrated fibres of the shell of Mytilus occurred here as at Sparebakken, and when dried on the sieve formed sheets like paper.

We collected the following species, in addition to those given by Sars:—

Terebratula cranium,	. 1
Tellina solidula, .	. 1
Lepton nitidum, .	. 2
Leda pygmæa, . .	. 1
Montacuta bidentata,	. 4
„ ferruginosa,	. 1
Axinus flexuosus, .	. 1
Scrobicularia alba, . .	. 1

Thracia papyracea, *var.*,	3
Chiton cinereus,	6 plat.
,, ruber,	6 ,,
Patella vulgata,	1
Scissurella crispata,	1
Trochus helicinus,	2 frag.
Rissoa punctura,	common.
,, parva,	1
,, ,, *var.* interrupta,	9
,, striata,	common.
Skenea planorbis,	2
Homalogyra atomus,	common.
Cæcum glabrum,	,,
Odostomia unidentata,	4
,, interstincta,	9
,, spiralis,	1
Purpura lapillus,	3
Utriculus truncatulus,	1
Velutina communis,	2
Echinus Cumbraensis,	2 plat.
Verruca strömia,	plates.
Balanus porcatus,	2 plat.
,, crenatus,	common.
Cliona celata.	

ENTOMOSTRACA.

Cythere viridis, *Müller*.
,, villosa, *Sars*.
,, lutea, *Müller*.
,, pellucida, *Baird*.
,, angulata, *Müller*.
,, cuneiformis, *Brady*.
Encythere argus, *Sars*.
Loxoconcha tamarindus, *Jones*.
Sclerochilus contortus, *Norman*.
Cytheropteron nodosum, *Brady*.
,, latissimum, *Norman*.
Cytheridea papillosa, *Bosquet*.
Xestoleberis depressa, *Sars*.
Cytherura undata, *Sars*.
,, nigrescens, *Baird*.
,, lineata, *Brady*.
,, cellulosa, *Norman*.
,, acuticosta, *Sars*.
,, striata, *Sars*.

FORAMINIFERA.

Polymorphina lactea, *W. & J.*
Quinqueloculina seminulum, *Linn.*
Truncatulina lobatula, *W. & J.*
Nonionina depressula, *W. & J.*
Polystomella striato-punctata, *F. & M.*
Triloculina oblonga, *D'Orb.*
Quinqueloculina subrotunda, *Montagu.*
Rotalia Beccarii, *Linn.*
Nonionina scapha, *F. & M.*
,, asterizans, *F. & M.*
Lagena sulcata, *W. & J.*
,, globosa, *Montagu.*
Dentalina brevis, *D'Orb.*
Bulimina ovata, *D'Orb.*

Bisœt, near Christiania.

This is a true clay, situated in the immediate neighbourhood of Christiania, and is classed as post-glacial from the abundance of *Isocardia cor*, while Tellina calcarea (proxima) also occurs. The co-existence of T. calcarea and Isocardia cor indicates a transitional condition of climate, and is very remarkable. Isocardia cor has been dredged by Lilljebord in Molde fjord, near Christiansund, and it ranges as far south as Sicily, while T. Calcarea is a characteristic arctic shell, extinct except in high latitudes.

A gradual change of climate appears substantiated, therefore, by the fact of the co-existence of these shells during the post-glacial epoch.

Fragments of Tellina calcarea occur with the valve and teeth firmly clenched together, the shell being broken away at the hinge. Some specimens have the valves crushed together, like examples of the same shell in the Gamrie deposit, Banffshire.

The clay filling many of the shells is different in colour from the clay in which they are imbedded, and is quite hard and not soluble

in cold water. Of the clay generally, 8 lbs. when washed leaves only 1 oz. residue.

We collected the following species, in addition to those given in the lists of Prof. Sars:—

Nucula tenuis, . . 2
,, ,, *var.* inflata, 1
,, ,, nucleus, . 1
Leda pygmæa, . . 8
,, ,, *var.* gibbosa, 3
Scrobricularia nitida, . 1
Rissoa striata, . . 1
,, soluta, . . . 1
Odostomia interstincta, . 1
Natica Grœnlandica, . 1 you.
,, Alderi, . . 1
Cerithium reticulatum, . 1
Cylichna nitidula . . 1
Utriculus truncatulus, . 1

Vertebra of fish.
Amphiodotus—spine.

ENTOMOSTRACA.

Cythere tuberculata, *Sars.*
,, Dunelmensis, *Norman.*
Cytheridea punctillata, *Brady.*
,, papillosa, *Bosquet.*
,, Sorbyana.

Loxoconcha tamarindus, *Jones.*

FORAMINIFERA.

Nonionina scapha, *F. & M.*
,, turgida, *Will.*
Polystomella striato-punctata, *F. & M.*
Rotalia Beccarii, *Linn.*
Bulimina marginata, *D'Orb.*
Biloculina elongata, *D'Orb.*
Lagena distoma, *P. & J.*
,, gracillima, *Seguenza.*
,, striata, *Montagu.*
,, lævis, *Montagu.*
Rotalia orbicularis, *D'Orb.*
Truncatulina lobatula, *W. & J.*
Quinqueloculina agglutinans, *D'Orb.*
Triloculina trigonula, *Lamk.*
Bulimina marginata, *D'Orb.*
,, aculeata, *D'Orb.*
Lituola scorpiurus, *Montfort.*
Dentalina brevis, *D'Orb.*
,, communis, *D'Orb.*

Barholmen near Drobak.

This beautiful island is in the Christiania fjord, and contains one of the most remarkable deposits we ever examined. The island rises to the height of about 100 feet above the sea; and at various points, from the sea level to the summit, the shell beds are exposed.

In the lower part of the deposit, extending from the sea to about 30 feet of elevation, the shells are characteristic of very deep water, and belong to the Finmark fauna. Lima excavata is very plentiful, and attains a large size. Pecten aratus, P. vitreus, Terebratula cranium (generally broken valves), also occur in great abundance.

The bed is composed of a sandy clay; and throughout the whole, *Oculina prolifera* is plentiful.

Prof. Sars states that this coral is found living on the north and west of Norway, never at a less depth than from 150 to 300 fathoms. It occurs fossil, attached to the rock at the bottom of the sea, off Barholmen, at a depth of from 60 to 90 fathoms, and continues to pervade the beds to the summit of the island (100 feet).

From these facts, Prof. Sars argues that there must have been an upheaval of the land to the extent of at least 700 feet, 650 feet being

the shallowest depth which can be assigned for the habitat of the living coral.

We obtained beautifully preserved masses of Oculina prolifera on the shore. They were fossil, and had been brought up by fishermen from the reef. We also collected fragments from every part of the island.

We collected the following species in the 30 fee bank, in addition to those given by Prof. Sars:—

Terebratula Spitzbergensis,	5 valv.
Anomia patelliformis, *var.* striata,	1 ,,
Anomia ephippium, *var.* squamula,	1 ,,
Nucula nucleus, *var.* tumidulus,	1 ,,
Leda pygmæa,	1
Axinus ferruginosus,	2 frag.
Venus ovata,	1 ,,
Mya truncata,	2 sm. frag.
Corbula gibba,	1 frag.
Lepeta cœca,	abun.
Propilidium ancyloides,	30
Scissurella crispata,	abun.
Trochus Grœnlandicus,	1 you.
Rissoa comicoïdes,	2
,, albella,	3 frag.
,, Jeffreysii,	abun.
Odostomia insculpta,	2 you.
,, unidentata,	3
Eulima distorta,	1 you.
Defrancia teres,	1
Pleurotoma Trevellyana,	1
Utriculus obtusus,	1
,, truncatulus,	1
Cylichna nitidula,	6
Spirialis retroversus,	abun.
Siphonodentalium subfusiforme,	4

ENTOMOSTRACA.

Paracypris polita, *Sars.*
Cythere crenulata, *Sars.*
Cythere pellucida, *Baird.*
,, viridis *Müller.*
,, villosa, *Sars.*
,, lutea, *Müller.*
,, echinata, *Sars.*
Cytheridea punctillata, *Brady.*
Ilyobates prætexta, *Sars.*
,, Bartonensis, *Jones.*
,, glacialis,
Loxoconcha tamarindus, *Jones.*
Cytherura striata, *Sars.*
,, angulata, *Brady.*
Cytheropteron alatum, *Sars.*
,, latissimum, *Norman.*
,, punctatum, *Brady.*
,, Montrosiensis, *nov. sp.*
,, nodosum, *Brady.*

FORAMINIFERA.

Biloculina ringens, *Lamk.*
Lagena lævis, *Montagu.*
,, sulcata, *W. & J.*
Dentalina communis, *D'Orb.*
Polymorphina lactea, *W. & J.*
Bulimina ovata, *D'Orb*
,, elegantissima, *D'Orb.*
,, marginata.
,, aculeata.
Planorbulina Haidingerii, *D'Orb.*
,, Ungeriana, *D'Orb.*
Truncatulina lobatula, *Walker.*
Pulvinulina repanda, *F. & M.*
The large punctulate crag variety.
Nonionina umbilicata.
Nonionina turgida, *Will.*
Lagena lævis, *Montagu.**

The highest bed at Barholmen (100 feet) contains more littoral shells, while its composition is more sandy. Here we find Littorina littorea, and Littorina rudis, the former being so plentiful that it may be taken as a characteristic shell. Intermixed with these are many of the same species as in the lower beds, although the deep sea forms are scarcer and more broken. We have here we believe, therefore, the

* One of the specimens double.

remains of the shore of the sea, in the deep waters of which the lower bed was deposited.

We collected the following species, in addition to those given by Prof. Sars, from the same spot:—

Terebratula cranium,	frag. abun.
,, caput serpentis	,, not rare.
Anomia ephippium, *var.* aculeata,	3
Ostrea edulis,	4
Pecten opercularis,	frag.
,, septemradiatus,	1 valv.
,, tigrinus,	1 frag.
,, striatus,	2 ,,
,, aratus,	4 ,,
Lima elliptica,	4 ,,
Mytilus phaseolinus,	1 you.
,, modiolus,	1 ,,
Modiolaria discors,	5 ,,
Nucula nucleus,	15
Leda pygmæa,	2
Arca pectunculoides,	1
,, nodulosa,	6
Lepton nitidum,	1 frag.
Montacuta bidentata,	5
Axinus flexuosus,	1 frag.
Cardium exiguum,	3
,, minimum,	3
,, edule,	5
Astarte sulcata,	37
,, borealis,	3 you.
Venus lincta,	4 frag.
,, ovata,	2 ,,
Corbula gibba,	1 broken.
Saxicava rugosa,	abun.
Scrobicularia piperata,	1
Tapes aureus,	1
Pholas candida,	1 frag.
Dentalium abyssorum,	
Chiton ruber,	9 plat.
,, cinereus,	8 ,,
Tectura virginea,	20
,, fulva,	21
Propilidium ancyloides,	1
Puncturella Noachina,	1
Emarginula crassa,	3
Capulus Hungaricus,	6
Scissurella crispata,	3
Trochus millegranus,	3
,, tumidus,	1
Lacuna divaricata,	7
Littorina limata,	3
,, obtusata,	1
Rissoa Jeffreysii,	9
,, punctura,	22
,, Zetlandica,	1
Rissoa parva,	18
,, inconspicua,	2
,, violacea,	2
,, albella,	1 frag.
Homalogyra atomus,	9
Cæcum glabrum,	abun.
Odostomia interstincta,	3
,, acicula,	1
,, Scillæ,	1
Eulima distorta,	2
,, bilineata.	
Natica affinis,	4
,, Montacuti,	11
Velutina lævigata,	2
Aporrhais pes pelicani,	2
Cerithium perversum,	3
Cerithiopsis tubercularis,	3
Buccinum undatum,	frag.
Trophon clathratus,	5 ,,
,, truncatus,	8 ,,
,, Barvicensis,	6 ,,
Nassa pygmæa,	1 ,,
,, incrassata,	6 ,,
Columbella nana (Loven)	1 ,,
Defrancia linearis,	4 ,,
,, ,, *var.* equalis,	1 ,,
Pleurotoma costata,	4 ,,
,, Trevellyana,	1 frag.
Cylichna nitidula,	4 ,,
Utriculus truncatulus,	2 ,,
,, obtusus,	1 ,,
Spirialis retroversus,	7 ,,
Echinus milliaris,	spines.
Spatangus purpureus.	
Ray plate of Ophiura.	
Cleona celata.	
Otolite of small fish.	

ENTOMOSTRACA.

Cythere lutea, *Müller.*
,, villosa, *Sars.*
,, crenulata, *Sars.*
,, badia, *Norman.*
,, angulata, *Sars.*
,, viridis, *Müller.*
,, pellucida, *Baird.*
Cytheridea punctillata, *Brady.*
,, papillosa, *Bosquet.*
Loxoconcha impressa, *Baird.*
,, tamarindus, *Jones.*
Cytherura nigrescens, *Baird.*

Cytherura undata, *Sars.*
Cytheropteron latissimum, *Norman.*

FORAMINIFERA.

Polymorphina horrida, *Rss.*
Nodosaria pyrula, *D'Orb.*
Truncatulina lobatula (fragment), *W. & J.*
Dentalina communis, *D'Orb.*
Uvigerina angulosa, *Will.*
Vaginulina legumen, *Linn.* (frag.)
Lagena sulcata, *W. & J.*
Lagena globosa, *Montagu.*
Pulvinulina elegans, *D'Orb.*
„ repanda, *F. & M.**
Nonionina asterizans, *F. & M.*
„ depressula, *W. & J.*
Rotalia Beccarii, *Linn.*
Bulimina elegantissima, *Will.*
„ ovata, *D'Orb.*
„ marginata, *D'Orb.*
Polymorphina lactea, *W. & J.*
„ compressa, *D'Orb.*
Lagena distoma, *P. & J.*

Pladsen Trondstad on Haaoen near Drobak.

This is of the same age as the preceding. The clay bank rests against the rock from about 30 feet to within 3 or 4 feet of tide mark. Twelve pounds of clay lost by washing 9½ lbs., leaving 2½ lbs. of residue, made up of sand, gravel, and broken shells. Over the clay are patches of sand and gravel.

We collected the following species in addition to those given by Prof. Sars:—

Terebratula cranium,	1 frag.
Leda pygmæa, *var.* gibba,	3
Lepton nitidum,	1
Montacuta bidentata,	1
Cardium fasciatum,	8
Scrobicularia nitida,	2
Saxicava rugosa, *var.* arctica,	2
Chiton cinereus,	4 plat.
Emarginula fissura,	3
Natica Grœnlandica,	9
Trochus cinerarius,	1
„ millegranus,	1
Lacuna divaricata,	1
Rissoa punctura,	1
„ parva, *var.* interrupta,	1
„ inconspicua,	1
„ membranacea,	1
Odostomia turrita,	1
„ spiralis,	1
Natica affinis,	2
Nassa pygmæa,	1
„ incrassata,	1
Defrancia linearis,	2
Trophon Barviciensis,	1
Admete viridula,	1
Verruca strömia,	2 plat.
Ray plate of star-fish.	
Echinus Dröbachiensis,	spines and plates.
Cliona celata,	1

ENTOMOSTRACA.

Cythere Dunelmensis, *Norman.*
„ angulata, *Sars.*
„ viridis, *Müller.*
„ concinna, *Jones.*
Cytheridea papillosa, *Bosquet.*
„ punctillata, *Brady.*
Ilyobates Bartonensis, *Jones.*
Cytheropteron nodosum, *Brady.*
Cytherura undata, *Sars.*
„ nigrescens, *Baird.*

FORAMINIFERA.

Lagena gracillina, *Seguenza.*
„ distoma, *Parker & Jones.*
„ gracilis, *Williamson.*
„ striata, *Montagu.*
„ sulcata, *Walker & Jacob.*
„ *var. nov.*
„ lævis, *Montagu.*
„ apiculata, *Reuss.*
Dentalina communis, *D'Orbigny.*

* The very large variety found in the "Crag" of Suffolk.

Cassidulina, lævigata, *D'Orb.*
Pulvinulina elegans, *D'Orb.*
Nonionina depressula, *W. & J.*
" asterizans, *Fichtel & Moll.*
" scapha, *F. & M.*
Polystomella striato-punctata, *F. & M.*
Truncatulina lobatula, *W. & J.*
" refulgens, *Montfort.*
Bulimina marginata, *D'Orb.*
" aculeata, *D'Orb.*
" orata, *D'Orb.*
Glandulina lævigata, *D'Orb.*
Nodosaria (apparently a single joint of N. Boueana, *D'Orb.*)
Uvigerina pygmæa, *D'Orb.*
" angulosa, *Will.*
Biloculina depressa, *D'Orb.*
Quinqueloculina seminulum, *Lamarck.*
Dentalina obliquestriata.
Polystomella crispa, *Linn.*
Triloculina trigonula, *Lamk.*
Lagena globosa, *Montagu.*

Comparing the post-glacial beds of Norway with those of Scotland, we indicate, as suggestions for further inquiry, the following results:—

1. The arctic species of the glacial epoch seem to have lingered longer in Norway than in Scotland, since some of them appear in the post-glacial beds now described, while they are absent in any corresponding beds in Scotland which we have yet observed. Possibly some of these Scotch beds now classed together as "glacial" may indicate a transition in the proportions in which glacial species occur; but in the Scrobicularia piperata clay near Montrose, the Cardium edule bed at Paisley, the Ostrea edulis bed at Stirling, and other corresponding post-glacial deposits, there is no admixture of arctic species. At many points on the banks of the Clyde the Pecten maximus bed rests upon the glacial clay, but contains no purely arctic shells. Probably, therefore, while at the period represented by the beds at Errol (Scotland) and Moss (Norway), the climate in both countries was very similar, the amelioration took place more rapidly in the West of Scotland.

2. The deep sea post-glacial bed at Barholmen has no precise equivalent in Scotland; but in Scotland the term "raised beach" does not cover the facts, and post-glacial beds of different depths also occur. Cyprina islandica, for example, and other shells which are not littoral, abound in the banks of the river Irvine, in clay beneath a deposit of pure shore sand with strictly littoral shells. On both countries therefore, there are post-glacial beds, indicative of different depths of water.

VII.—*On the Recovery of Sulphur from Alkali Waste.* By MR. LUDWIG MOND.

Read in the Chemical Section.

ATTEMPTING to give you a short description of my process for the recovery of Sulphur from Alkali Waste, I beg in the first place to ask your pardon for calling your attention to a simple manufacturing process of so little general interest, on the first meeting of this assembly, which might certainly have been occupied by a worthier subject; and still more for my boldness in addressing you in your own language, which I have by no means sufficiently at my command. I will, however, do my best to bring before you, as briefly as possible, the few points of interest which my subject may offer, and hope you will be kind enough to excuse the many deficiencies which I am unable to escape.

It will be unnecessary for me to explain to an assembly of Glasgow gentlemen the vast importance of the alkali trade, you having within your walls, at St. Rollox, the most important and most interesting alkali works in the world,—the most important and most interesting not only on account of their vastness, but because a considerable number of the most valuable improvements in the manufacture of alkali and its branch industries have originated or been first adopted in these works, several of which are still the property of the firm, and carried out at their premises only.

The process for the manufacture of bleaching powder, which has now become so extensive that it can hardly be called a secondary product, was invented by the founder of the firm, and is still carried out in their works on a scale at least double that of any other works in the world. The production of chlorine from nitrate of soda, and the recovery of manganese, are inventions of their late manager, Mr. Dunlop, and are still specialties of the St. Rollox works; and among other improvements which have been first introduced here, I may name specially the apparatus for the methodical lixiviation of black ash, of which I shall have to treat more fully this evening, as it was on this invention that the final success of my process altogether depended; and it is certainly remarkable that the first apparatus ever used for the lixiviation of black ash is at present employed in the recovery of sulphur by my process. Yet many of you will be astonished at the enormous development of the alkali trade within the last few years.

In 1864 the quantity of salt decomposed in Great Britain amounted to 288,000 tons, and within three years it has risen to fully 400,000 tons per annum, or nearly 40 per cent. The transformation of this quantity of salt into sulphate of soda requires about 320,000 tons of oil of vitriol, which contain nearly 100,000 tons of sulphur. This large quantity of vitriol is at present almost entirely obtained by burning iron and copper pyrites, containing from 36 to 50 per cent of sulphur. Thanks to the exertions of Messrs. Charles Tennant & Co., who have been the principal promoters of a company formed for the purpose of purchasing, and connecting by railway with the sea, the Tharsis mine, near Huelva, one of the richest mines in Spain (which promises to furnish the total supply of pyrites for Great Britain for any length of time); and also owing to the intelligence and perseverance of another of your fellow-citizens, Mr. William Henderson (who has brought his process for the extraction of copper from these ores, after the sulphur has been burnt off, to such an unprecedented pitch of perfection); the sulphur in these ores is now supplied at a price which places England in as favourable a position as any other country in respect to this important raw material, and which makes the use of native sulphur in the manufacture of salt cake almost impossible. Although the large quantity formerly used for this purpose has within the last twenty years been almost altogether replaced by pyrites, the export of brimstone from Sicily, whence hitherto has come all the supply of this valuable material, has been continually rising, and has now reached over 200,000 tons per annum, about 50,000 tons of which are consumed in Great Britain.

This shows that brimstone has found other fields of employment, even more readily than it has lost its former place; and we see here again, that in our time of rapid progress, new raw materials, or new processes, will generally make their way, and give beneficial results, without doing any harm to the trades which they have been originally intended to supplant.

Besides the manufacture of pure sulphuric acid, which we are still unable to obtain from pyrites by a really practical method, and some other industries of minor importance, the largest quantities of brimstone are now converted into rolls and flowers of sulphur, used in vineyards and hop-gardens for suppressing the much-dreaded diseases of those plants, and in the manufacture of gunpowder, the consumption of which must certainly have reached very large proportions, since all nations have exerted their best energies to find out the most effectual method of using this substance for their mutual destruction. The indispensability of sulphur in the manufacture of gunpowder, even

makes it a question of political importance for any nation not to be altogether dependent on another country for the supply of a material so necessary for the defence of its freedom and independence; and this question has certainly become more grave since Italy has been formed into a strong and united kingdom. The appearance of a few English men-of-war would now-a-days hardly be sufficient to re-open the ports of Sicily for the exportation of brimstone, as it has been on a former occasion. I hope to be able to show you that the repetition of such a scene will no longer be necessary, and that England is now able to obtain the brimstone which she imports from a material which has hitherto been worse than valueless, being only a source of inconvenience and outlay to the manufacturers, who could not help producing it; and that this brimstone can be produced at a cheaper rate even than at the mines in Sicily.

Ever since the illustrious Leblanc, more than eighty years ago, introduced his famous process for the manufacture of alkali from common salt, which (excepting only a few hundred tons made from cryolite) still furnishes all the vast amount of alkali at present used, and which in all probability will continue to do so for many years more, it has been known to chemists and manufacturers, that nearly all the sulphur used in this manufacture remained in the insoluble residue which is left after having separated the alkali by lixiviation from the crude soda, or black ash, obtained by melting sulphate of soda with limestone and coal.

This residue, well known under the names of alkali waste, blue waste, or tank waste, is produced in very large quantities, 1½ tons of dry, or above 2 tons of wet, waste being formed for every ton of alkali manufactured. It accumulates in the vicinity of alkali works, in heaps and hills of sometimes enormous size, which create a considerable nuisance, by exhaling in damp weather volumes of sulphuretted hydrogen, the disagreeable qualities of which need no comment, and by giving rise to the formation of yellow liquors washed out by rain, and containing sulphides of calcium, which have a very deleterious effect on wells and rivers to which they gain access. A large number of distinguished scientific and practical men have, for the last thirty years, spent much time and labour in the task of recovering the sulphur thrown away in the waste (which now amounts, as already stated, to nearly 100,000 tons a year in Great Britain alone, and represents a considerable value), and doing away by this means with the nuisance referred to, at the same time.

Before all others, it is Mr. William Gossage to whose ardent labours we owe so many valuable improvements in the manufacture of alkali,

who will always be named in connection with this subject for the brilliancy of his ideas, and his perseverance in carrying them out. Though unsuccessful, like most others, he cleared the way for future investigations; and I gladly state that I derived much benefit from conversations with him whilst I worked out the details of my present process.

The problem of obtaining sulphur from the waste was a comparatively simple one. The great difficulty, however, was to treat the large bulk of waste without incurring too much expense for plant and wages. The wet waste containing no more than 12 per cent. of sulphur, which has an average value of say £6 per ton, it will be obvious that only a very simple method, which requires small outlay to start with, and but little expenditure for repairs and labour, would satisfy the primary condition of a manufacturing process—viz., to pay well for the invested capital. I believe I have ultimately overcome these difficulties completely. The waste is treated by my process without being removed at all, and the wages are thus cut down to a very low amount.

The total outlay for plant is generally covered by the savings of one year, and the maintenance of the apparatus is very trifling. These are the practical points of the process which have made it successful, where so many others, working in the same direction, have failed, and where, consequently, the merits of the process in comparison with others must be principally looked for.

My process is based on the following principles:—The conversion of the insoluble compound of sulphur and calcium in the waste into soluble compounds by oxidation, the separation of these soluble compounds by lixiviation, and the ultimate recovery of the sulphur from the solutions thus obtained, by muriatic acid.

When I started my researches in connection with this process in 1860, I was not aware that these re-actions had been formerly proposed for the same purpose. A few years ago, however, I found that they had already been laid down, before I was born, in the specification of a patent of Mr. W. H. Leighton, in October, 1836. This interesting document, entitled "Improvements in Converting Sulphate of Soda into Subcarbonate of Soda," relates principally to a very impractical construction of a black ash furnace, with a perforated floor through which steam should be introduced. At the end of his paper the author states —"I allow the waste to remain in the vats until it heats and gives off smoke. I then lixiviate it, and obtain liquors from which I separate sulphur by muriatic acid." It is, however, very improbable that he has ever worked out this part of his specification, as no account of it is to be found except in the register of patents, and as it escaped the notice

of all other investigators of the subject. Though nearer to my method than any later suggestion, it was quite impracticable, as it would have required a very long time and an enormous plant, owing to the oxidation proceeding very slowly and not being at all under control, and would have given very small returns. From the date of this patent to the time of the starting of my researches, only one process has been published for utilizing the products of oxidation of alkali waste.

This relates, however, solely to the manufacture of hyposulphite of soda, and was patented by Mr. W. S. Losh in 1852. He oxidized the waste in heaps, lixiviated, mixed the liquors obtained with a solution of carbonate of soda, settled and boiled the cleared liquors down to crystallization.

This process has ever since been worked very successfully at the Walker Alkali Works, near Newcastle, with which Mr. Losh has been connected, and he was certainly the first who oxidized waste on a large scale in order to utilize it. By his mode of oxidation, as Mr. Clapham, the present manager of the said works, has kindly informed me, about one-fifth of the sulphur contained in the waste is obtained in solution—a quantity altogether insufficient to make it available for sulphur recovery; and this may probably have been the reason why Mr. Losh never thought, or certainly never mentioned, that his liquors might be employed for this purpose.

During the time that I was carrying on my investigations, and before they were brought to a close, no less than three patents have been taken out on the same subject; the first of which was granted to two well-known fellow-citizens of yours, Mr. J. Townsend and Mr. James Walker, and is dated 11th December, 1860. These gentlemen, after having oxidized and lixiviated the waste in exactly the same way as Mr. Losh, proceed to convert all the sulphur in the liquors thus obtained into hyposulphite, by exposing them to the action of air in towers or otherwise, and then mix them with the cheaper sulphate of soda instead of carbonate of soda; thus producing at the same time sulphate of lime, containing a little hyposulphite, which they propose to sell to papermakers under the name of precipitated antichlore. Further, they suggest mixing the liquors obtained from the waste with the liquors obtained in making chlorine from manganese and muriatic acid, and separating by these means sulphur more or less mixed with sulphide of iron and sulphide of manganese, according to the proportions used. This last suggestion, and the use of sulphate of soda in place of carbonate of soda in the manufacture of hyposulphites, were valuable points in this patent. They, however, as well as the two other patentees referred to, Mr. A. Noble and Mr. J. L. Tullion, whose speci-

fications contain nothing new worth mentioning, have to all appearance given most of their attention to the production of hyposulphites, and have never taken up the question of sulphur recovery in earnest. All three oxidize and lixiviate the waste only once, according to the methods of Messrs. Leighton and Losh, and consequently their processes would not yield any more sulphur than former ones, a quantity so small that it would never have repaid the cost of erecting plant, and which certainly did not deserve the name of sulphur recovery.

If any of them had worked at the matter at all, he could not have failed to make the same observations to which I was led by my first experiments.

In studying the conditions under which the largest possible amount of soluble sulphur compounds could be obtained by the oxidation of waste, I very soon found that these compounds only increased up to a certain point, and would slowly decrease on continuing the exposure to air any longer.

When, however, the soluble compounds at their highest points were removed by lixiviation, the waste would yield, by a second oxidation, the same quantity as before, and even a third repetition of this treatment would give nearly as good results. Thus, by oxidizing and lixiviating the waste three times, I was able to extract from the waste three-fifths of the sulphur which it contained, the remaining sulphur being nearly all in the form of sulphate and sulphite of lime, both insoluble; but which, far from being noxious, made the waste a valuable manure, owing to its containing, besides the gypsum, very large quantities of carbonate of lime and caustic lime, as well as soluble silica and a little soda. These experiments were carried out at an old-established alkali work at Ringkuhl, near Capel, in Germany. At the time named, the black ash was lixiviated there by an old method, which has since been abandoned, according to which it was several times removed from one vessel to another. This caused the waste to be so very dense, that all my efforts to oxidize it in heaps, as by forcing air through it, failed completely, so that I could only obtain the results mentioned by exposing it to the air in shallow layers on shelves.

Having never had to deal with other alkali waste, I naturally concluded that the same would be the case with the waste of other works, and took out patents for this mode of working in 1861 and 1862, after having successfully treated considerable quantities of waste in the works named.

Coming to England in 1862, I was soon convinced that this process was quite impracticable for the treatment of the vast quantities of waste there produced, and would be fearfully expensive at the rate of

wages there current, which was fully three times as high as in the works I came from. I also noticed the vast difference in the condition of the waste produced by the excellent mode of lixiviation already noticed, according to which mode the waste remains, so to say, suspended in liquor and water until it is spent, and is thus left very porous. As soon as I found an opportunity, I tried again to oxidize it by forcing air through the waste, and this time I succeeded so well that the time of oxidation, which was previously fourteen days, was reduced to as many hours, so that there was now a possibility of treating the waste in the same vessel in which it was made, without increasing the plant to an unreasonable size, and thus altogether avoiding the labour for transporting and manipulating the large mass of material, which has always been the greatest difficulty of the question. Having fully satisfied myself about the practicability of this method, by experimenting with vessels holding 10 tons of waste, I took out a patent for it, dated the 8th September, 1863, since when the process has undergone no change in its essential features.

The apparatus for the lixiviation of black ash, so often referred to, consists of a set of four to six vessels, mostly called vats, which stand on a level, and are connected by pipes in such a way that the water entering one vat will pass from the bottom of it to the top of the next, and so on. They are fitted, besides, with taps and shutes to convey the strong solution of alkali to settlers. By this arrangement the water always comes in contact with black ash very nearly spent, and the weak solution obtained from this is made to pass through black ash containing more and more alkali, until at the end they meet fresh black ash, and consequently become strongly concentrated. At the same time, the black ash remains continually covered, or so to say, suspended in liquors of different strengths, and the waste is thus obtained very porous. Formerly, this waste was cast out of the vats as soon as the alkali was washed out, and the vats were then re-filled with black ash. I, however, proceed to oxidize and lixiviate the waste in the same vats, so that it is not removed until the obtainable sulphur as well as the alkali are washed out. As a matter of course, I want for this purpose an additional number of vats, which, however, amounts to six extra vats only for every set of four as generally in use. Thus a set of ten vats is formed, which are all connected in the usual way, but are provided in addition with extra pipes, taps, and shutes, for the passage of the liquors obtained by lixiviating the oxidized waste, and which we will call sulphur liquors, through the several vats in order to obtain them more concentrated, and finally into reservoirs or cisterns. All the vats have also perforated false bottoms, the space underneath

which is connected with a fan by means of pipes with dampers, which latter allow of the regulation of the oxidation.

As soon as the alkali is completely washed out of the waste, and the last weak alkali's liquor is drained off, the damper in the windpipe is withdrawn, and air forced through the waste for a certain time, which varies according to the pressure of the blast, the porosity of the waste, and the desired composition of the liquors to be made.

The exact time of exposure and pressure of blast which will yield the best results must be ascertained by experiments for almost every work separately; generally an exposure of 12 to 16 hours, and a pressure of 3 to 4 inches of water, being sufficient.

On the admission of air the waste soon begins to heat, the temperature rising gradually to 200° Fahr. It gives off quantities of steam, turns quite greenish, but of a bright yellow colour on the top, then becomes more and more dry, and would finally take fire if the blowing was continued sufficiently long.

When the proper state of oxidation is arrived at, which the workmen will soon know by experience, and see pretty well from the appearance of the waste, the blast is stopped, and the soluble products are washed out with water. This operation can be readily performed in 6 hours. The blast is then again turned on for the same length of time as before, and the waste lixiviated, and the same treatment repeated a third time.

As you see, from the numbers already given, this treatment altogether requires from 60 to 72 hours only. The sulphur liquors are allowed to pass several vats filled with oxidized waste, by the connecting pipes already alluded to, and are thus obtained concentrated.

They contain from 5 to 7 per cent. of sulphur, principally in the form of sulphides and hyposulphites. The separation of the sulphur from these liquors by means of muriatic acid, which appears to be so very simple, was attended with a good deal of quite unexpected difficulty. During the first year of working the process, I carried the oxidation of the waste so far as to obtain liquors containing principally hyposulphite and only little sulphides. These latter I converted into hyposulphite by passing sulphurous acid through the liquor, and then added muriatic acid, and boiled in order to obtain from the hyposulphite sulphur and sulphurous acid. The latter was utilized for converting the sulphides of another portion of liquor into hyposulphite.

I obtained, however, a very impure sulphur, containing so much gypsum that it could hardly be melted, and I found it very difficult to obtain sulphurous acid in sufficient quantity for the first part of the process, though the original liquor contained only one-fourth of the sul-

phur in form of sulphides. These inconveniences, and the considerable loss of sulphur in form of gypsum, induced me to abandon this method, and to adopt the following:—

The oxidation of the waste is so regulated that the liquors obtained contain just sufficient oxygen, in the form of hyposulphurous acid, to oxidize the calcium and hydrogen present, in the form of sulphide of calcium and sulphydrate of calcium, into oxide of calcium and water. These liquors are run into vessels of wood, brick, or stone, simultaneously with an equivalent quantity of muriatic acid, the liquid in these vessels being kept continually at a temperature of 140° to 150° Fahr. The exact proportion of liquor and acid, which may of course be precisely ascertained by analysis, can be sufficiently well judged, from the colour of the mixing liquids, by an experienced workman. I will try whether I am able to show you these colours, and to separate some sulphur from the liquid in this bottle, which is of the composition above referred to, though it is very difficult to manage a process of this sort on a small scale. By this mode of working, about 90 per cent. of the sulphur contained in this liquor is precipitated in a very pure state.

It settles very quickly, the clear supernatant solution is run off, and the sulphur drawn out by a door at the bottom of the precipitating vessel, into a wooden vat with double floor, where the remaining chloride of calcium is thoroughly washed out with water. It is then dried, and melted by heating it in an iron pot, and is thus obtained in the lumps which are before you on the table, and in a purer state than the average brimstone imported from Sicily.

As you will see from the different numbers above mentioned, fully one half of the sulphur contained in the waste is thus obtained in a very pure and marketable form; and the alkali waste yearly produced in Great Britain could furnish nearly the full quantity of brimstone which is now imported, without falling back on the tremendous quantities of waste stored up in the vicinity of old works.

Sometimes the latter may be used very advantageously to increase the yield of sulphur without going to increased plant and labour. Water being brought in contact with these heaps, as is often done by nature in the form of rain or ground water, dissolves out yellow liquors, which contain principally sulphydrate of calcium and very little polysulphide. These may be mixed with liquors rich in hyposulphite, or they may be used in lieu of water, as has been preferred by Messrs. C. Tennant & Co., to lixiviate the oxidized waste, which in this case is of course to be oxidized so as to contain more hyposulphite than usual.

By these means the amount of recovered sulphur has been increased to fully three-fourths of the quantity contained in the waste treated by my process. Where muriatic acid is scarce—which, however, is very seldom the case where it is properly economized—the waste liquors from the manufacture of chlorine, containing free hydrochloric acid, free chlorine, perchloride of iron, and chloride of manganese, may advantageously be used for the separation of sulphur, as has been pointed out before me by Messrs. Townsend and Walker. This mode of working is exactly as with muriatic acid.

The free acid, the free chlorine, and one-third of the chlorine present in the form of perchloride of iron, are thus utilized, and the mixed liquors, containing principally chloride of calcium, and manganese, and protochloride of iron, are colourless and comparatively harmless.

Thus a second waste and offensive product can be advantageously disposed of.

The sulphur obtained is, however, in this instance not very pure, containing some sulphide of iron, and does not settle so readily as when made by muriatic acid.

With regard to the theoretical part of my subject, I am very sorry not to be able to give you a satisfactory account of all the re-actions taking place at the different stages of my process.

We have to deal with a class of bodies, the polythionic acids, which have, for some unaccountable reason, been very little studied, though they certainly promise very interesting results of great value to theoretical chemistry.

They yield so little to the theories put forth of late years, which, being principally based on our increased knowledge of organic bodies, have left many well-known facts of inorganic chemistry unexplained, that I prefer to use old equivalents in explaining to you the few facts which I have been able to ascertain, as these will allow the re-actions to be represented by simpler formula. I regret very much that my time has never allowed me to enter this subject *as fully as I should have wished to do.* Like most manufacturing chemists, being engaged in business, I could not attend to sincere theoretical work. I believe, however, that my process, though still very young, is not in respect to the theoretical questions involved in it worse off than its elder brethren, taking into consideration that our knowledge of the theory of our most important manufacturing processes is still very incomplete, and indeed amounts to very little more than the fact, that the substances which we produce have been contained in the raw material used, while we know so very little of what the manufacturer really wants for his guidance—viz., the different stages through which those raw materials

go—that we must mostly depend on practical experience alone in conducting those processes. You may perhaps believe this statement to be exaggerated.

Leaving, however, the processes of destructive distillation of organic substances, so largely employed for manufacturing purposes, and the manufacture of cyanides and ferrocyanides, out of the question, I will only refer to the manufacture of ultramarine (of which we do not even know the formula), to the chamber process for making sulphuric acid, and to Leblanc's method for the production of alkali, the different stages of which are by no means satisfactorily established, as a few illustrations of this fact. This state of things is certainly astonishing at a time when our science has made such rapid progress in every direction; and I cannot help thinking that scientific men must look with a sort of contempt upon researches on manufacturing processes,—as I cannot otherwise explain, that out of the hundreds at present working in every branch of the science, so very few take up these important questions, but prefer to employ their time to prepare all possible salts of a new element, or to study innumerable homologues of a series of organic compounds.

Returning to our subject, we have, in the first place, to consider the action of air and moisture on alkali waste. The principal constituent of the waste is a compound of calcium and sulphur, insoluble in water, the exact composition of which is still a matter of dispute—some considering it as oxysulphide, others, simply as monosulphide of calcium.

I have not been able to form a definite opinion on this question; as both would, however, behave in the same way, it is simpler for our purpose to take the latter view.

CaS in contact with HO is converted into $CaHS_2$ and $CaHO_2$. This re-action, which goes on very slowly in the cold, is accelerated by the heat evolved during the oxidation of the waste; the carbonic acid of the air, with CaS, also forms $CaHS_2$ and $CaCO_3$.

The oxygen of the air oxidizes the CaS to CaS_2 and CaO, the latter of course combining with the water present, in moist waste.

At an early stage of a slowly-conducted oxidation, we thus obtain liquors from the waste containing nearly only CaS_2 and $CaHS_2$.

Continuing the action of air, or forcing it through the waste at a greater speed, the $CaHS_2$ is oxidized into HO and CaS_2, and the CaS_2 formed both by this and the above-mentioned re-action is further converted into CaS_2O_3. This latter compound decomposes readily, when exposed to an elevated temperature, into S, and $CaSO_3$, which

is very insoluble. The sulphur thus separated is taken up by the CaS_2 in the solutions, forming polysulphide, which again dissolves $CaHO_2$ out of the waste, forming with it compounds only sparingly soluble, which sometimes separate from the liquors in beautiful crystals. Forcing air at a moderate speed through a certain height of waste in a vat, all these re-actions will take place at the same time in different places, and the liquors obtained by lixiviating after certain periods of exposure, will contain the substances named in quantities proportionate to the time of exposure. By forcing the air rapidly through the waste, and for a greater length of time, the CaS_2 will be nearly all converted into CaS_2O_3, and thus liquors are obtained very suitable for the preparation of hyposulphites. By going still further, the CaS_2O_3 will completely decompose into $CaSO_3$ and S. The former will rapidly form $CaSO_4$, with the evolution of a considerable quantity of heat, which will cause the sulphur to take fire and burn to sulphurous acid, which partially escapes. The temperature being once so elevated, the CaS, which remained so far unoxidized, is converted rapidly into $CaSO_4$, and the heat in the waste thus rises sometimes so far as to make it red-hot. You will see from these re-actions that the prolongation of the exposure to air over a certain limit will cause decomposition of soluble sulphur compounds in some parts of the waste at first, and will gradually turn them altogether into sulphate of lime, which would make them lost for our purpose. Removing them, however, at a proper time prevents this loss to a great extent, and enables us to treat the waste repeatedly with the same result. So far I have, however, not been able to prevent the oxidation of a part of the CaS into $CaSO_3$ and $CaSO_4$, and this is the reason why I cannot recover all the sulphur which the waste contains, by my method. I have, however, no doubt that, with a few years' more practical experience, the process will in this respect yield better and better results, as has been the case with every other manufacturing process known.

The sulphur liquors contain, as already stated, according to the time of exposure and the pressure of the air, varying quantities of CaS_2O_3, CaS_x, $CaHS_2$, and CaO, HO, besides a trace of $CaSO_3$ and $CaSO_4$, which are however both so insoluble (in 800 and 400 parts of water respectively) that they hardly deserve to be noticed. It is of great importance to ascertain the relative quantities of the first-named constituents, as the oxidation must be regulated accordingly. I have found no means to ascertain the quantity of CaO, HO in these liquors, as in presence of $CaHS_2$ it behaves exactly as CaS. As it,

however, plays the same part during the decomposition of the liquor, it makes no difference to our purpose to determine and calculate it as present in this form.

The hyposulphite is determined as usual, by a standard iodine solution and starch, after having precipitated the sulphides by a salt of zinc, and filtered. Another portion of the liquor is tested by the same iodine solution and starch, until the blue colour appears. It is then again decolorized by a drop of hyposulphite of soda solution, litmus is put in, and a standard caustic soda solution is added until the liquor remains blue. The addition of acetate of soda makes the end re-action of this process very distinct.

The following re-actions take place:—

$$2\ CaS_2O_3 + I = CaI + CaS_4O_6$$
$$CaS_x + I = CaI + {}_xS$$
$$CaHS_2 + 2\ I = CaI + 2\ S + HI$$
$$HI + NaO = HO + NaI.$$

As you will readily see from these equations, the iodine used for the first test shows the calcium present in form of hyposulphite; the caustic soda, the calcium present in form of sulphydrate; and from these and the total iodine used, the calcium present in form of polysulphide is easily calculated.

The latter containing generally only little more than 2 equivalents of sulphur to 1 of calcium, the test may be as well applied to ascertain approximately the amount of sulphur present in the liquor. In separating sulphur from these liquors in the way before described, and carried out in your presence, though the final result of the process is so very simple—

$$CaS_2O_3 + 2\ CaS_x + 3\ HCl \text{ forming } 3\ CaCl + 3\ HO + (2 + 2\ x)\ S,$$
$$\text{and } (2 + 2_x)\ S, \text{ and}$$
$$CaS_2O_3 + CaHS_2 + 2\ HCl \text{ forming } 2\ CaCl + 3\ HO + 4\ S.$$

There are very likely all possible compounds of hydrogen, oxygen, and sulphur, from HS_5 to HSO_4, formed in different parts of the vessel, and at different stages of the process.

I have been able to ascertain the presence of all those of the compounds named which our present knowledge allows us to recognize in presence of the others—thus, HS_5, HS, HS_2O_3, HS_3O_6, HS_5O_6, HSO_3, and HSO_4. Of the two missing ones, HS_2O_6 and HS_4O_6, we have at present no re-actions which would allow their presence to be ascertained when mixed with the others. The formations of HS_5 and HS, by bringing in contact CaS_5 and $CaHS_2$ with HCl, is obvious to

every one. It takes place only in starting an empty vessel, and then the HS_5 is often seen floating on the top of the mixing liquors. The formation of HS_5O_6 very likely takes place by the direct decomposition of CaS_2O_3, as Chancel and Diacon have directly obtained it by decomposing BaS_2O_3 with HSO_4 ($5\ CaS_2O_3 + 5\ HCl = 5\ CaCl + 2\ HS_5O_6 + 3\ HO$). The HS_3O_6 is formed by the following equation:—

$$5\ CaS_2O_3 + 3\ HCl = 3\ CaCl + 3\ HO + 2\ CaS_3O_6 + 4\ S.$$

These I found to be the principal products of the decomposition of CaS_2O_3 and HCl at a temperature of 140° to 150° Fahr. On boiling, or by standing for a number of hours, the CaS_3O_6 decomposes into $CaSO_4 + S + SO_2$, which accounts for the large quantities of gypsum obtained by decomposing liquors very rich in CaS_2O_3.

If, in this latter case, the muriatic acid is poured into the liquor gradually, the SO_2 thus formed converts the CaS_2O_3, still present, again into CaS_3O_6 ($2\ CaS_2O_3 + 3\ SO_2 = 2\ CaS_3O_6 + S$), and this is again decomposed into $CaSO_4 + S + SO_2$, so that the end of the re-action would be very nearly

$$3\ CaS_2O_3 + HCl = CaCl + HO + 2\ CaSO_4 + 4\ S.$$

This accounts for the difficulty which I found in obtaining sulphurous acid in quantities by decomposing hyposulphite of lime by means of muriatic acid. In my present mode of working, however, the CaS_3O_6 formed meets again with CaS_2 which reduces it to CaS_2O_3 ($CaS_3O_6 + CaS_2 = S + 2\ CaS_2O_3$), the CaS_5O_6, or any other CaS_xO_6, undergoes a similar reduction, and thus the formation of gypsum is almost entirely avoided. Only at the end of an operation there remains a certain small quantity of hyposulphite of lime in the liquors, which is decomposed by adding a quantity of muriatic acid insufficient to saturate the calcium in this hyposulphite, and the clear decomposed liquor, which is run off our precipitating vessels, thus contains, besides chloride of calcium, all the acids mentioned, though in very small quantities.

I must here leave my subject, asking you again to excuse the manner in which I have brought it before you, on account of my being a foreigner to your country and language. I shall be very happy to give you any further explanation on those points which I have failed to make sufficiently clear.

VIII.—*On Animal Charcoal, particularly in Relation to its Use in Sugar Refining.* By WILLIAM WALLACE, PH.D., F.R.S.E.

Read in the Chemical Section.

ANIMAL charcoal is made by heating bones in retorts until the organic substances they contain are decomposed into volatile matters and carbon, the latter remaining mixed with the mineral constituents of the bones. The manufacture is carried on at Mr. Townsend's works at Port-Dundas, Mr. Poynter's in Greenock, Messrs. Macfarlane and Arthur's in Paisley, and Mr. Brown's at Carntyne, Duke Street. Nearly all the bone-char made in these works, besides a large quantity imported from France and Russia, is employed in the sugar refineries on the Clyde. The quantity manufactured is considerable; for although charcoal lasts a very long time, yet it requires to be occasionally renewed; and the stock gradually dwindles down by various causes, to which I shall presently refer, and must therefore receive constant though small additions. Without claiming to be quite precise, I calculate the quantity of charcoal in use in the Clyde refineries at from 4,000 to 5,000 tons—probably 5,000 tons is nearer the mark than 4,000—while the annual renewal is probably about 1,500 tons.

The carbonization is effected in cast-iron cylinders of about 18 inches diameter and 10 feet long; but the form and size may be varied at pleasure. The time occupied varies also according to circumstances; but twelve hours is a common length of time, and generally makes better charcoal than six hours, even although the heat in the latter case is made stronger. The retorts are kept going day and night as long as they are fit for use. The manufacture in many respects resembles that of coal gas; and there are formed, as in that process, gases and liquid substances, while a solid remains behind containing the mineral matter mixed with carbon.

The bones, after being charred, are crushed between rollers, and the dust removed, when the charcoal is ready for use. The quality of the char varies a little with the description of bones employed and the care taken in the manufacture. For example, there are what are called home-collected bones, which certainly, if hand-picked, make the best charcoal; there are South American shank bones, from the salideros of Brazil and Buenos Ayres; and there are camp bones, which are

frequently dug up from old battle-fields, and must be a mixture of all sorts, but chiefly cattle and horses. Many of these bones bear evidence of having been buried for a considerable time; and it is quite easy to distinguish the charcoal made from them from that prepared from recent or "home-collected" bones. Besides these varieties, we have large shipments from Italy and Turkey, in the latter of which the bones of the camel are mixed with those of cattle, antelopes, sheep, and horses.

The composition of the best charcoal, made from home-collected bones, is something like this, *when dry* :—

Carbonaceous Matter,	10
Phosphates of Lime and Magnesia,	18
Carbonate of Lime,	8
Sulphate of Lime,	·2
Alkaline Salts,	·8
Oxide of Iron,	·1
Silicic Acid,	·3
	100

The carbon varies a little according to the amount of boiling with water the bones have received, in order to remove grease and gelatine; but it also varies in different parts of the bone, the exterior hard part having a smaller percentage than the interior and softer portion. Hence the larger the size of the charcoal, the less the proportion of carbon present. The carbonate of lime also varies a little. Besides the above ingredients, the charcoal usually contains about 10 per cent. of water, which is thrown upon it in order to cool it, and to prevent the carbon from burning away.

The chemical analysis of charcoal presents certain difficulties, notwithstanding the apparent simplicity of its composition. This is particularly the case with regard to the carbonic acid. But I will not detain you by describing my method, as you will find one equally good, although perhaps a little more complicated, in the second volume of Fresenius, special part. In this process, as in my own, the carbonic acid is accurately measured. The method used by some chemists of precipitating the phosphate of lime, and then throwing down the lime in the filtrate in the usual way, gives results entirely fallacious. There is occasionally a trace of free lime in the charcoal; but I have never found it to be more than a minute quantity. It is best, however, before estimating the carbonic acid, to treat the finely pulverized charcoal with solution of carbonate of ammonia.

I have stated carbon to be one of the constituents of charcoal; but

although it is always called carbon, it is not strictly pure carbon, but consists partly of that element and partly of nitrogen. The fact that charcoal prepared from animal matters contains some nitrogen has long been known; but I do not recollect having seen any statement of the amount. I have made only a few determinations myself; but these appear to indicate that the proportion is variable, and also that, when the char is used in sugar refining, the amount constantly diminishes. Thus, in a sample of char made from home-collected bones, I found no less than 1·55 per cent. of nitrogen out of a total of 8·5 of carbonaceous matter; and in another sample, made from foreign bones, I found 1·08 of nitrogen out of 9 of carbonaceous matter.* Two samples of moderately old char gave respectively ·3 and ·55 of nitrogen—the quantities of carbonaceous matter being respectively 15 and 17. I am not at present prepared to express an opinion whether this nitrogen plays an important part in the decolorizing action exerted by charcoal; but one thing is certain, and that is, that we know of no variety of charcoal that is really a good decolorizing agent that is not made from a highly nitrogenous substance. Wood charcoal, for instance, although eminently porous, is a very poor decolorizing agent, and is practically useless. I shall, however, reserve my remarks upon the active constituent of charcoal until I have discussed some other points.

New charcoal always contains traces of ammonia; and we cannot wonder at this when we consider that a large quantity of ammonia is given off from the bones during the calcination. The amount is usually very minute.† Frequently it exists as sulphide of ammonium, and in this case causes great damage to sugar run through it; but such accidents are entirely prevented by washing the charcoal well, and re-burning it before passing liquor over it. By this treatment the common salt is removed, together with the traces of ammonia; traces also of sulphide of calcium are either removed or rendered harmless. Whenever charcoal is overburned, the small quantity of sulphate of lime always present is decomposed, forming sulphide of calcium; and this always exerts an injurious action upon sugar. Such overburned charcoal gives off the odour of sulphuretted hydrogen when moistened with water, and still more distinctly when treated with an acid. A

* The carbon in this sample, separated by dissolving in acid, gave 8·2 per cent. of nitrogen, or in 9 grs. ·74, or only about $\frac{2}{3}$ of the total quantity of nitrogen present. In other cases the proportion of nitrogen in the separated carbon was much less. Generally, the quantity of nitrogen in new charcoal is about one-tenth part of the total carbonaceous matter.

† In a particular case I found a new charcoal to contain ·011 per cent. of ammonia.

sample of new char gave ·08 per cent. of sulphuretted hydrogen on treating with hydrochloric acid.

Again, charcoal, both new and old, retains appreciable quantities of gases which escape when cisterns containing it are filled with liquor; and these gases are frequently combustible, and, when mixed with air, explosive.

I shall now refer to some mechanical properties of animal charcoal. I have long regarded the bulk that charcoal occupies, as compared with its weight, as a property of great importance. A ton of new charcoal, when dry, fills a space of 48 or 50 cubic feet; but the same weight of old charcoal occupies a much smaller space—it may be 40, 35, 30, or even so little as 28 cubic feet; in fact, its *apparent* density, when measured dry, increases with age until it is nearly double what it was at first. But when we come to the absolute specific gravity of old and new char, we find that the difference is very slight indeed. Thus, a sample of new charcoal, occupying 50·6 cubic feet per ton, or having an apparent gravity of ·71, had a real specific gravity of 2·822; a moderately old sample, occupying 35 cubic feet per ton, or having an apparent gravity of 1·03, had a real gravity of 2·857, or only a trifle more than the new charcoal. The specific gravity of phosphate of lime in a pure state is about 3, that of carbon about 1·8 to 2, and carbonate of lime 2·7; and the gravities found correspond very closely with the calculated numbers. Now, here is a point of the utmost importance, and one to which I believe I was the first to draw the attention of sugar refiners—that charcoal diminishes rapidly in bulk by constant re-burning, while the real gravity remains practically unaltered. What are we to infer, then? Evidently this, that, by constant re-heating, the particles of charcoal become smaller, by the diminution of the pores; and hence old charcoal is less porous than new; and hence, also, the apparent gravity of char furnishes a ready and certain indication of its value for sugar-refining purposes. The mere effect of the continued application of heat is well illustrated in the following experiment:—A quantity of new char was taken, occupying 48 cubic feet per ton, and exposed to a pretty strong heat in a covered crucible for an hour, after which the space taken up by a ton was only 43·2 cubic feet; after two hours more it was 40·8 cubic feet; after other four hours it was 38 cubic feet; and after four hours longer of a strong heat it was 35·5 cubic feet: thus losing in eleven hours as much porosity as it would by being re-burned in a sugar-house about 100 times.

It is well known to chemists that phosphate of lime or bone-ash is capable of being readily fused at a high temperature, such as that of a

pottery kiln, and that in fact it is largely employed in potteries for making what is called English porcelain, in which it acts the part of felspar in the best French and German porcelain; that is, it fuses, and, binding the particles of clay and flint together, makes the whole vitreous and non-permeable by liquids, while common earthenware is quite porous unless glazed. A bone pretty strongly ignited becomes, not exactly fused, in the ordinary sense of the word, but the particles appear to agglutinate, and the bone shrinks very much in volume, retaining its shape, and is now vitreous and even translucent. When struck, it has almost the ring of a piece of metal. I have no doubt that a still higher temperature would actually fuse the bone; but I have never seen the experiment carried so far. In the case of animal charcoal which, in re-burning, is subjected only to a moderate degree of ignition, the phosphate of lime does not fuse, but it is evidently acted upon to such an extent that its particles agglutinate, and the pores become lessened. The statements I have made regarding the apparent gravity of old and new charcoal amply prove this; but the same conclusion may be arrived at by other means. A piece of new charcoal burnt white has the dull porous fracture of chalk; but when old char is burnt it has the appearance of pieces of flint or pebbles, showing conclusively that the pores in it are either smaller or less numerous than in new charcoal. Again, if we take some new charcoal in a cistern or a funnel, and run water upon it until it refuses to retain any more, we find that it will hold a very large quantity—viz., from 80 to 100 per cent.—that is, if the charcoal is previously quite dry. But if we make the same experiment with old charcoal, we find a marked difference, the quantity capable of being retained being only from 30 to 45 per cent. Once more, new char, if quite dry, requires about 20 per cent. of water to make it perceptibly wet, while old charcoal (two or three years in use) requires only about 5 per cent. These facts prove incontestably that the pores of old char are either smaller or less numerous than in new char; and I think it is a fair conclusion that, if we wish to retain char in an efficient state, we should burn it so as to lessen the porosity as little as possible.

But although the action of heat is the main cause of the increase in the apparent gravity of char, it is not the only one. In most sugar-houses the proportion of carbon gradually increases; so that, beginning with a charcoal containing 9 or 10 per cent. of carbon, we have it in two or three years increasing to 14, 15, or as I have even seen it, although in an extreme case, 19 per cent. The source of this carbon is obvious enough: it is derived from the organic impurities extracted from the sugar, which in re-burning are carbonized. But this carbon is not

deposited upon the surface of the charcoal, at least not exclusive and hence the accumulation fills up the pores to a certain exte Charcoal containing a large deposit of carbon is always more or l glazed; but the glazing depends in part, at least, upon the friction which the char is exposed. Perfectly new char may be glazed simple attrition of its own particles, but the process is tedious. T more carbon there is in old char, the more readily does it becon glazed by attrition.

The deposit of carbon being a very great evil in sugar refining, it obvious that if it can be prevented, a point of great importance gained. That this can be done is certain; for in some refineries tl carbon does not increase at all, and in others it speedily diminishes, that sometimes it does not exceed 2 or 3 per cent. But when decrease takes place, it is from a false system of treatment,—either tl admission of air into the re-burning apparatus, allowing the hot char pass through the air into the cooling boxes, or the application in tl kilns of too high a temperature, which causes a re-action to take pla between the carbon and the elements of water, resulting in the form tion of carbonic oxide gas and carburetted hydrogen. But if ever thing is tight, and the heat not excessive, the carbon will inevitabl increase, unless we take the precaution of washing out of the charco before re-burning, nearly all the organic matters which have bee absorbed from the sugar liquor. This can only be done by a lar expenditure of water, which should be quite boiling. Indeed, one our most advanced sugar refiners insists that the charcoal must l *boiled with the water;* and my own experience quite bears out h assertion.

But extensive washing does more than simply remove the vegetab matters absorbed from the sugar. There are mineral matters also a sorbed, not from the sugar only, but also in some cases from the wat used to dissolve the sugar. In all sugars we have a certain proporti of soluble salts of potash, soda, lime, and magnesia, the total quanti being in ordinary raw cane sugars from ½ to 1 per cent.; but in be very much more, varying, in the qualities that come here for refinin purposes, from 1½ to 3 per cent., although in extreme cases amounts to as much as 6 or 7 per cent. The highly soluble salt such as those of potash, have no effect upon the charcoal, and onl annoy the refiner by accumulating in the syrups; but sulphate of lim a salt only very slightly soluble in water, has a very serious influenc Sulphate of lime dissolved in sugar liquor is readily absorbed b charcoal; but it may be got rid of by extensive washing, and mo completely still by boiling. The power char possesses of absorbin

and retaining certain substances depends very much upon circumstances. So long as the sugar solution is strong (having a gravity of 1·2 or 1·25), sulphate of lime is absorbed and retained; but whenever the washing begins, and the liquor gets weak, the sulphate of lime commences to dissolve out, so that it is no uncommon thing, in boiling down weak char washings, to obtain a plentiful crop, not of sugar, but of sulphate of lime. When the water is hard and contains much sulphate of lime, the proper washing of charcoal becomes almost, if not quite, an impossibility; and I have myself analyzed charcoal containing $2\frac{1}{2}$ per cent. of sulphate of lime; while in the refineries on the Clyde it is a very extreme case when 1 per cent. is present, the usual amount in old charcoal being from $\frac{1}{2}$ to $\frac{3}{4}$ per cent. Hard waters also contain usually a good deal of lime in the form of bicarbonate, or carbonate dissolved in solution of carbonic acid; and this carbonate is readily absorbed by charcoal, at least when it is abundant, and sometimes fills up the pores to such an extent as to render the char practically useless. I shall revert to this subject by and by.

Let us now turn for a brief space to the offices fulfilled by the various ingredients of charcoal. And, first, let us inquire to which of its constituents does it owe its properties, and what are these properties? Charcoal, that is, animal charcoal, has a powerful affinity for—or at least, as that phrase may be objected to, a great power of absorbing—gases, colouring matters, and mineral salts, which are only slightly soluble in water; all of these being in solution. It is with colouring matters that we have chiefly to do in sugar refining; but not entirely so, for raw sugar contains vegetable albumen and various gummy and other matters, which for convenience are grouped under the general term of extractive matter; and these are as readily absorbed as the true colouring matter, and their removal is quite as essential. So that if we could practically bleach sugar by sulphurous acid, chlorine, ozone, or some other chemical agent, we should still require to use charcoal to get rid of the extractive matter. I have found by experiment that ordinary egg albumen is absorbed in large quantity by charcoal, and also gum of the ordinary soluble kind; and it is remarkable that both of these substances have an insoluble modification, and I am inclined to think that this has something to do with their absorption. Again, charcoal, at least when not too old, readily absorbs a considerable quantity of iron, so that liquors contaminated with that metal are completely freed from it by passing through a cistern of new char.

The ingredient which exerts this powerful action is evidently the nitrogenous carbon; for if we burn charcoal perfectly white, not only on the outside of the grains, but to the very centre of each particle, it

no longer retains *the slightest trace* of decolorizing power. This I have proved by actual experiment, and is not a mere opinion. But it is quite evident that the carbon owes its extraordinary powers to its extreme porosity, the particles being infinitely comminuted and kept asunder by admixture with ten times their weight of phosphate of lime. The effect produced upon sugar liquor is truly wonderful. Although of a dark reddish-brown colour when run on to the char, it comes away, for a time at least, perfectly colourless. After running for some time it begins to lose its power—in other words, the pores become more or less saturated—and the liquor gradually acquires, first, a pale lemon yellow, and then a brownish colour. The sugar refiner takes care to economize his char as much as possible by passing through it, first, good raw sugar, afterwards an inferior sort, and lastly syrups from the drainage of previous refines.

But although the carbon is the essential decolorizing ingredient, yet there is another substance in charcoal that exerts a very marked influence upon the process of sugar refining, and that is carbonate of lime. This ingredient is exceedingly useful in neutralizing the small proportion of free acid present in almost all sugars, with the exception of beet; and it is still more important on account of its neutralizing the lactic and other acids formed in the weak liquors during the washing of the charcoal, by a process of fermentation which it is very difficult to prevent. Hence charcoal which has been deprived of all or nearly all its carbonate of lime is very objectionable, and is sure to give rise to sour liquors and the occurrence of iron in the lower classes of sugar produced in the refinery. When the water used contains only traces of carbonate of lime, as is the case in Greenock and Glasgow, the proportion of that compound naturally present in the charcoal constantly decreases, until, in pretty old char, it is reduced to about 1½ per cent. It seldom goes below this, and never so far in a refinery conducted upon scientific principles; but I have seen char that did not contain any appreciable quantity. When the quantity falls below 2½ per cent. sour liquors are sure to follow. On the other hand, when very hard water is used, even although sour sugars are used, the carbonate of lime either decreases very slightly or it increases, and sometimes to an alarming extent; and in beet refineries on the Continent, where lime is freely added to the juice, the evil is a very serious one. In this case it closes up the pores, and does a great deal of damage in various ways; and many expedients have been adopted for the purpose of getting rid of it. The old process consisted in treating the char, after re-burning, with 1 or 2 per cent. of ordinary hydrochloric acid, diluted with enough water to wet the char completely. But in this

case the saturation, although pretty rapid, was not sufficiently so: the char which came first into contact with the acid got too much of the acid, which dissolved some of the phosphate of lime; and when the liquid got to the char furthest from the surface it was completely neutralized, and did no good whatever. The process of Mr. Ed. Beanes is a very great improvement upon this system, and has been adopted with marked success in restoring old charcoal which had been used in beet factories, or in refineries where very hard water was employed. It consists in saturating the perfectly dry char with hydrochloric acid gas (dried by means of chloride of calcium), afterwards exposing it to the air until the excess has escaped, and then washing it well with water and re-burning. The expense of the process is considerable, as two washings, dryings, and re-burnings are required, over and above the cost of the acid. The use of this process in refineries where the water is not hard, or when circumstances do not require its aid, is attended with very disastrous consequences, and even its constant and continued use in any case is a great mistake, as it is impossible to get rid, by washing, of every trace of free acid; and this exercises a marked effect upon the crystallization of the sugar, and serves also to permit the syrups to become contaminated with iron when kept, as they usually are, in iron tanks. On the other hand, it must be allowed that a trace of acid in sugar improves the colour, making it very perceptibly paler or whiter. This is readily seen on adding a drop of any acid, or even vinegar, to a yellow syrup, when the colour is immensely improved. If, however, the syrup is to be boiled down, the acid does far more harm than good, and should certainly be avoided.

Beanes's process and others of a similar nature have likewise been applied with marked success to new charcoal, for the purpose of removing traces of ammonia and free lime, and also a part of the carbonate. When a refinery starts afresh with new charcoal entirely, the use of acid to improve the char is attended with the best results; but when only 5 or 10 per cent. of new is added to the old char, it does probably more harm than good. That char deprived of its carbonate of lime by acid gives finer liquor than ordinary char, either new or old, is quite undeniable; indeed, the difference is very marked; but upon the whole the best practical results, throughout the whole of the operations of a sugar-house, are obtained when the charcoal contains about 3 or 3½ per cent. of carbonate of lime, and the liquors are as nearly as possible neutral. When a fine yellow colour appears in the crushed sugar produced, as is commonly observed after a liberal addition of new char to the old, it is an indication of the presence of a trace of alkali; while, on the other hand, sugar from very old charcoal, nearly free

from carbonate of lime, has always more or less of a gray tint, and a sour disagreeable smell, and such sugar always contains traces of iron.

Another process has been proposed for treating charcoal with hydrochloric acid—that of Mr. Gordon—in which the char is placed in a cylindrical vessel of considerable size, which is then exhausted by an air-pump, after which dilute hydrochloric acid is allowed to rush in at a great many points, so as to saturate the char at once. The advantage of this over Beanes's process is that we can in this way apply any given quantity of acid, while with the gas we must always use excess. Hydrochloric acid, besides removing carbonate of lime, likewise serves to dissolve out some of the sulphate of lime; but this can also be done by washing plentifully, or, still better, by boiling with water.

I have as yet referred only to the decolorizing power of charcoal; but it has another property which, although valuable when the article is used for purifying water and as a deodorizer, is a very serious drawback to its use in sugar refining—I mean its power as an oxidizing agent. If water containing oxidizable organic matter and free oxygen is digested with animal charcoal, or passed through a cistern of it, the organic matter is oxidized by the free oxygen, and the water is rendered purer and more wholesome. The same property can be illustrated in a variety of ways; but the one I have stated is sufficient for our purpose. When we begin to wash char in the cisterns of a refinery, after the syrup has drained away, we have a dilute solution of sugar formed which, in contact with the charcoal and the free oxygen contained in the washing water, becomes more or less oxidized, forming lactic and probably other acids, of which the nature has not as yet been particularly examined. I do not mean to say that we have here direct oxidation of sugar by free oxygen—for that is not the fact—but we have oxidation and alteration of the nitrogenous matters extracted by the char from the sugar: these undergo fermentation, and then re-act upon the sugar, the ultimate result being the formation of certain organic acids. These acids make the washings sour and putrid; but this is not the only evil, for they at once decompose any sulphide of calcium or sulphide of iron in the charcoal; they dissolve carbonate of lime, sulphate of lime, and oxide of iron; and the consequence is that the char washings become so very impure that, when thrown back amongst the other products of the refinery, or mixed with a fresh lot of raw sugar, they cause an immense amount of mischief, and in certain cases have been the means of bringing the operations of the refinery to an abrupt conclusion.

This is a department of sugar refining which, from its importance,

has necessarily occupied a good deal of my attention; and I think I may say that I have made known to refiners, at least to such of them as have thought fit to occupy themselves with the subject, the means of entirely preventing such injurious results. The method recommended is simply this:—While the sugar liquor is on, the char cisterns are kept up to at least 150°,—a temperature sufficient, so long as the liquor is strong, to prevent any fermentation; then, when water is applied for the purpose of washing down the sugar, it is run on quite boiling; in fact, if it has been kept boiling for some time before it goes into the char cistern, so much the better; and so it is kept boiling so long as any of the char washings are to be preserved. When these directions are attended to, there is never any difficulty with sour char washings, or with the presence of iron in the lower-classed products of the refinery. If the char is afterwards washed continuously for ten or twelve hours with boiling water, it will do more good than cold water; and if boiled with water, as in Mr. Gordon's method of treatment, it is even better.

We now come to the re-burning of charcoal, a subject of very great importance to the refiner. The object to be attained is to carbonize the small amount of organic matter extracted from the raw sugar which has not been removed during the process of washing. In order to do this well, the process should be economical as regards fuel; it should allow of the complete carbonization of the organic matters; it should permit of the ready escape of the gases and vapours produced; and it should expose the charcoal for only the smallest possible length of time to the heat required for carbonization, so as to avoid the contraction of the pores of the charcoal by the action of heat, besides all the other evils attending overburning.

There are two distinct kinds of re-burners,—those in which upright pipes are used, and those that consist of horizontal revolving cylinders.

The kind of kiln in general use consists of a series of upright cast-iron pipes, generally arranged in six rows of eight or ten pipes each row, three rows being on one side of the furnace and three on the other. The flame of the furnace plays directly upon the pipes, and the gases are conducted away from the sides of the kiln. The wet char, as it comes from the cisterns, is placed upon the top of the kiln, and sinks gradually down as the burnt char in the pipes is allowed to fall into the cooling boxes below. These consist of sheet-iron vessels the same length as the row of pipes to which they are attached, about 6 feet deep and $\frac{3}{4}$ inch to 1 inch diameter, and cooled simply by contact with the exterior atmosphere. This kiln is open to many

objections. The wet char above prevents the free escape of the gases and vapours resulting from the carbonizing of the char below, and so these vapours are to some extent driven through the hot char downwards into the cooling boxes, from the joints of which combustible gases and steam may frequently be seen to escape, and where masses of ammoniacal salts are sometimes observed to collect. Again, the heat applied is generally far too great; but still it cannot be avoided, as without it the great quantity of moisture present would not be driven off. In fact, the great mistake arises from drying and re-burning in one operation and in one vessel. The relative proportions of units of heat required to dry and to re-burn a given quantity of charcoal has been determined by various authorities, but with very different results. Mr. Gordon gives the quantities as three to dry and one to burn; but my own experiments, conducted with great care, show that with ordinary average charcoal not more than one-fifth part of the total heat is required for re-burning, and with new char the proportion is still less. Now, in the upright pipes the char at the bottom is exposed to the strongest heat, and most unnecessarily so; the strongest heat should be applied where it is most wanted—that is, to the wet char at the top of the pipes. I must say, however, that this kiln has done much good service; and if it were improved, as it might be, it would still be used with advantage. The gases and vapours can be drawn off readily by a series of inverted funnels moved up and down by machinery. This has been patented by Mr. Gordon, and has been found to work admirably in his own sugar-houses. Then, again, the char might easily be wholly or partially dried before being introduced into the pipes—a process which has been adopted successfully in some refineries. Lastly, the cooling boxes may readily be made of such depth (say 10 feet) that the char leaves them almost quite cold; or water may be employed in cooling.* With these improvements well carried out, the common pipe kiln might still successfully compete with others of greater pretensions, and the amount of work performed by them might easily be increased to three times the present quantity.

What are known as Chantrell's kilns are constructed of blocks or slabs of fire-brick, and are a very clumsy and expensive modification of the iron pipe kiln. They possess no advantages over the latter, and are in some respects inferior, while they consume a very much larger quantity of fuel.

* The water is not applied to the char itself, but is contained in a series of pipes, or in a narrow chamber in the centre of each cooling box. The direct application of water to hot char is very injurious.

Of cylindrical kilns we have Cowan's, Torr's, Bringe's, Gordon's, and Norman's. Cowan's is the simplest, but probably also the most defective, and consists of a single cylinder placed horizontally, which is half-filled with char, and then kept rotating over a furnace until no more vapours are given off; after which it is stopped, and the char withdrawn red-hot into iron boxes. In this process a good deal of the char becomes white, gray, or brown, from loss of carbon; and this result arises partly in the cylinder, and partly when the char is withdrawn. In the cylinder the larger particles of the char constantly fall over first, and come in contact with the red-hot metal, and these are always overburned, while the smaller grains run the risk of not being burnt enough. In most of the later modifications of this apparatus two cylinders are used, one being placed above the other; and this exercises a considerable economy of fuel, the large expenditure of which in the case of the single cylinder is alone enough to prevent its adoption. In Mr. Gordon's process, patented in October, 1866, in which the re-burning, however, is only a part of the process, two cylinders are employed for *drying* the char, while the burning is effected in extremely narrow pipes, and occupies only a few minutes. But, after an extended series of experiments on a practical scale, he came to the conclusion that in any form of rotating cylinder the amount of waste by attrition is so great as to prove fatal to the process. He therefore adopted a singularly-formed kiln, being a combination of a pipe kiln in which the section of the pipes is 30 inches by 1 inch, and a kind of inclined plane, or rather series of steps, on which the char is dried before it passes into the pipes. In this process means are provided for drawing off and utilizing the steam and gases, and the char is introduced and drawn away continuously by self-acting machinery.

In Norman's kiln, recently patented, two cylinders are employed, each set at a slight incline, so as to make the char travel forward, and the char passes from one to the other, and finally out into cooling boxes of the usual construction. As in Gordon's original design, the cylinders are provided with ribs to carry round the charcoal, and means are provided for taking off, but without any attempt at utilizing, the steam and gases. This process works well in practice; does not consume more fuel, apparently, than the upright iron pipe kiln; and makes excellent charcoal,—although, like all other forms of revolving apparatus, it necessarily produces a large quantity of dust.

IX.—*On the Estimation of Potassium.* By JAMES CHALMERS Chemist, Kames Gunpowder Mills, and ROBERT R. TATLOCK, F.C.S., Analytical Chemist, Glasgow.

Read in the Chemical Section.

THE subject of the following paper is one of considerably greater importance than the above title would seem at first sight to indicate. When we reflect that Glasgow and its neighbourhood form the chief seat of the manufacture of potassium salts from kelp, and are the destination of the greater part of the "muriate of potash" imported into this country which is manufactured from the interesting deposit in the vicinity of Stassfurth, as well as of the potassium salts now so extensively made from French beet-root, it is sufficiently obvious that in the district in which Glasgow is situated, at least, an accurate and uniform method of estimating potassium is an indispensable adjunct to the manufacture and sale of these salts. That such a desideratum had long since been supplied might well be supposed, from the various methods of analysis described by different eminent authorities, and from the results of the experience of many able chemists incessantly engaged in the analysis of potassium salts. But the fact that serious discrepancies are constantly occurring with regard to the results obtained by different chemists of standing and experience, even when operating on the same carefully mixed and uniform sample, points to the conclusion that the instructions given by these authorities are imperfectly carried out, or that the details of the methods given are incomplete. Our own experience in the analysis of potassium salts having extended over a period of many years, during which we have made conjointly thousands of estimations of potassium, we have had very many opportunities of observation in connection with the subject.

By long and careful attention to the results of other chemists, confirmed by our own experience, we have invariably found that the general tendency is to report potassium too high; and the principal object of this paper is not only to trace the cause of this seemingly constant error, but to furnish, from the results of a laborious and protracted course of experiments, the true means of obviating that tendency, and obtaining constant and reliable results.

We are desirous of avoiding unnecessary strictures; but when we

find differences of 1 to 2 per cent. in analyses by different chemists, and results giving a total of 100 per cent. in a commercial "muriate of potash," from the potassium chloride and water alone, ignoring sodium salts (from which no commercial potassium salt is free), insoluble matter and potassium sulphate (a salt with a higher equivalent than potassium chloride), we are warranted in saying that serious errors are made.

These errors are usually discovered only when approximately pure salts are the subjects of investigation, from the fact of the analysis coming out impossibly high. It is almost superfluous to remark that any process which does not give from 99·9 to 100·15 per cent. with *pure* salts is inadmissible.

We admit that these errors and conflicting results are not necessarily the consequence of careless analysis or manipulation, but are in great measure owing to an unsuspected source of error in the principal re-agent employed. That these defects in "potash analyses" have so long escaped investigation and even general observation, is perhaps owing to a mistaken reliance on what is generally termed a "full analysis" of a muriate or other commercial potassium salt. It may be necessary to explain thus early, that although, in many cases of full analysis of compounds, when the sum of the various determinations amounts to 100 per cent., or very near it, the analysis is generally accepted as trustworthy, and deservedly so when each of the ingredients or elements are estimated separately, and not calculated from the amount of another element; yet, in the analysis of a commercial potassium salt, results approaching a total of 100 per cent. give no reliable check on the accuracy of a potassium determination, owing to there being no really *practical* method of determining sodium, even indirectly, in presence of potassium. Various methods are indeed given, but they require either extreme care and undue reliance on results (as by the method based on the comparison of the total chlorine with the weight of the total chlorides), or are only apparently direct (as in the separation and weighing of sodium in the form of chloride from the solution of sodium chloroplatinate). Hence we find that an error of 1 per cent. potassium too high is, by the usual method of analysis, at once so far compensated for by substituting ·59 less sodium, or practically, an error of 1·91 per cent. potassium chloride too high is disguised by omitting 1·50 sodium chloride. We thus obtain, it is true, a result ·41 per cent. too high in the case assumed; but from the fact that in commercial analyses chemists do not estimate such ingredients as exist in very minute quantities, and from irrecoverable losses, the analysis of commercial potassium salts ought to come out about ·3 per cent. too

low; hence such analyses, although erroneous by 1·91 per cent. of potassium chloride, or an equivalent quantity of any other potassium compound, will show a closer approximation to 100·00 per cent. than would a really perfect analysis.

The subject naturally divides itself into three distinct parts:—

I. The Chemical principles involved in the methods employed.

II. The Manipulation of the process; and,

III. The Calculation of the results.

I. *The Methods Employed.*

The method now almost exclusively practised for determining potassium and separating it from sodium is that by precipitation with platinum tetrachloride. This method is undoubtedly all that could be desired; but the details of it, as generally given, are meagre, and imperfect as regards accuracy: indeed, we know of no authority except Fresenius who habitually subjects analytical processes to a searching examination.

In most manuals it is directed to evaporate the solution containing the potassium compound to dryness with excess of platinum tetrachloride, and to digest the residue in alcohol before filtering. Most practised analysts, however, aware that this treatment is inapplicable to salts containing an appreciable quantity of sodium sulphate, properly avoid this source of error by stopping the evaporation somewhat short of dryness, and digesting the residue in strong aqueous solution of platinum tetrachloride, which dissolves sodium compounds, but practically leaves the potassium chloroplatinate intact. The other conditions requisite to ensure accuracy are various, and cannot be fulfilled without the exercise of much care. The most important of these is the *purity* of the *platinum chloride* solution, which is certainly the keystone of the entire process. Impure platinum and false results are as inseparably associated as crime and punishment. As platinum solution in a pure and fit state for analysis is not the rule, but the rare exception, it follows that erroneous results are alarmingly numerous; and it unfortunately happens that most, if not all, of the methods usually followed in the recovery of spent solutions and precipitates are the means of introducing impurities that are not readily removed. We shall proceed briefly to notice these methods, and to point out the objections to them, as founded on numerous trials.

The plans usually followed are four in number, and are as follows:—

1. Reduction by nascent hydrogen produced by the action of zinc on sulphuric or hydrochloric acid.

2. Reduction by alcohol in presence of excess of sodium hydrate.

3. Reduction by glucose or by cane sugar in a solution strongly alkaline by sodium carbonate.

4. Reduction by ignition of the precipitates and evaporated washings in a Hessian or other crucible, as recommended by Miller, Abel, and Williams.

All these methods we have tried extensively, testing the platinum solution in the most rigorous way, by making repeated estimations with *pure potassium chloride*—the article used being Griffin's, sold as chemically pure, and afterwards subjected to successive crystallizations by the granulating process, from distilled water. Fluorine, which, on account of its low equivalent, was the element whose presence was to be feared, at least in so far as high potassium results were concerned, was searched for, but not a trace found. High results could not be caused by the presence of sodium, on the other hand, that substance, even if present, not being estimated.

It may be as well to mention here, however, that the use of solution of platinum tetrachloride prepared from spongy platinum, purchased from a first-class manufacturer, and boiled in nitric acid and washed, before dissolving, gave, with the pure potassium chloride, results bordering on 102 per cent., using the equivalents accepted by certain practical authorities. These results are sufficient to show that the spongy metal, as purchased, is not in a fit state for preparing platinum solution for analysis, even after boiling in nitric acid.

We now proceed to notice the first-named method of recovering spent platinum compounds—namely, that by nascent hydrogen generated by the action of zinc on acids. We find it to be usually employed as follows:—

The spent platinum precipitates and solutions are diluted with water, and the mixture strongly acidulated with sulphuric, but preferably with hydrochloric, acid. Pieces of zinc are then added, heat is applied, and the fluid stirred till the whole of the compounds are reduced to metal. This is then washed with water, boiled in strong and afterwards in dilute nitric acid, washed again repeatedly, and dissolved in nitrohydrochloric acid, the excess of nitric acid employed being got rid of by evaporation two or three times with dilute hydrochloric acid. When this method was employed—commercial zinc being used—pure potassium chloride gave, with the resulting platinum solution, results far too high. We simply mention that they ranged from 101·67 to 102·05 per cent., and that similar results were obtained with potassium nitrate equally pure.

Using purified zinc,* the results were still too high, but a little nearer the truth. They were 101·37 and 101·58 per cent. of potassium chloride.

The chief objection we have to this method is the difficulty of procuring zinc free from lead and other impurities, the presence of which in platinum solutions is inadmissible, as, in cases where lead is present in the solution, its appearance in the precipitate is inevitable. In order to remove lead from our solution of platinum tetrachloride as much as possible, we evaporated the latter carefully to dryness, at 150° Fahr., and digested the residue in strong alcohol for sixteen hours. A distinct sediment of a brownish and pink hue remained, indicating lead. On evaporating again at 130° Fahr., to remove alcohol, a further small quantity of sediment was obtained. Platinum solution thus prepared gave in every case, with pure potassium chloride, results far too high, as follows:—101·15 to 101·49 per cent., using generally recognized factors. We may mention, in connection with this part of the subject, that one of us on one occasion obtained a crop of chloride of lead crystals, having a pink or rose tint, and weighing upwards of 90 grains, from the accumulated insoluble matters left on the filters in the filtration of platinum solutions prepared from the metal recovered by the zinc method.

With regard to the second method of recovering the platinum, we may observe that it is a modification of it that we have been in the habit of using for some time past in the preparation of the metal for use in the analysis of commercial samples. The following is an outline of the process as commonly followed:—The platinum waste is rendered strongly alkaline by solution of sodium hydrate, and is then boiled for some time with addition of alcohol. Reduction soon takes place, and the "platinum black" thus obtained is purified by boiling with nitric acid and with soda solution, with intermediate and final washings with water. With platinum thus prepared, high results were invariably obtained. The following are a few examples, working on pure potassium chloride and using recognized factors:—101·77 to 101·95 per cent., and 101·12 to 101·24 per cent., according to the degree of purification by the acids.

With reference to the third method of recovering—namely, that by glucose or cane sugar, along with a large excess of sodium carbonate, in

* This zinc was in the form of small rods, and was purchased as Griffin's best—free from arsenic—and sold for use in Marsh's test. It, however, contained cobalt in sufficient quantity to give minute films and crystals of a brownish purple colour on making a potassium determination, and to darken very considerably the colour of the platinum solution.

a boiling solution, as recommended by Böttger (*Chem. News*, vol. xi. p. 168)—we have to say that we did not get pure platinum by this mode, although the author affirms it should be obtained. Neither did we find any advantage by the use of glucose employed instead of the more easily-procured common white cane sugar, further than that the reduction took place more readily than with cane sugar. The process is objectionable, in so far as there is a very large and sometimes sudden escape of carbon dioxide gas, which is liable to cause frothing or boiling over; we also frequently find great difficulty in reducing the compounds to metal, which generally appears in a very fine state of division, rendering the washing and subsequent operations extremely tedious.

The results obtained in one set of analyses made with pure potassium chloride were from 100·76 to 100·94 per cent., using recognized factors.

These were so far satisfactory results, even when calculated by the factors used by authorities on the analysis of commercial potassium salts—data to which we shall be compelled to advert before concluding this paper. We need only remark at present that, calculating the last results by *Stas's* equivalents, we now obtained 100·22 to 100·40 per cent.

We may remark, finally, in connection with this part of the subject, that in this series of most careful trials, the manipulation of which was unchallengeable, in only four instances did we obtain results even approximating to the truth, with pure salts, by the use of platinum tetrachloride solution prepared from platinum which had not been *ignited.* The chief reason of this is, doubtless, the existence of organic compounds in the reduced metal, and which are not removed by the action of acids. This is not surprising when we consider the proneness of platinum to combine with organic matter, and the complexity of the re-action which takes place during the process of reduction.

We have now to turn to the fourth method of reduction, as recommended by Miller and Abel—namely, that by evaporating the platinum washings along with the precipitates, transferring to a Hessian crucible, and igniting strongly with free access of air, in order to burn off organic matters, the residual platinum being then washed and purified in the usual way, by boiling with hydrochloric and with nitric acid.

This method, as recommended, is imperfect in so far as that a greater change must occur than the merely burning off organic matter; for the decomposition of the platinic compounds must also take place. Now, to effect this, a very high temperature indeed is required:

we found a prolonged ignition—of at least an hour—in a good furnace necessary. So difficult is it to effect complete decomposition that, according to Fresenius (*Quant. Anal.*, 4th. ed., p. 100), even after long-continued exposure to a high heat, there remains a little potassium chloroplatinate, which resists decomposition, but which may be completely removed by ignition in a current of hydrogen, or with some oxalic acid. The statement as to the difficulty of reduction by ignition alone, we most readily agree with, as it accords with our experience. We therefore tried ignition along with some potassium nitrate, as indicated by Williams (*Chemical Manipulation*). Our first lot of platinum recovered in this way gave very unfavourable results, although the subsequent purification embraced boiling with hydrochloric and with nitric acid. Using Stas's factor (in practice, ·3056) for the calculation of potassium chloroplatinate to potassium chloride, to avoid high results in calculating, we obtained 100·89 and 101·02 per cent. with pure potassium chloride. A second quantity, by yet more intense ignition, gave, by an assistant, with Stas's factor, 99·88 and 100·13 per cent., and by ourselves 100·03 and 100·11 per cent., when in each case 6 drachms of water were used to dissolve the quantity of the salt operated upon, and which in each case was approximately 10 grains. A third quantity gave 100·16 and 100·22 per cent.; but in every case high results were obtained if less than 6 drachms of water were used—that is, if the precipitation was effected in concentrated solutions of the salt; or if it were, as is advised by authorities, dissolved in the "least possible quantity" of water; and even in very limited quantity the results were high.

Fair results—we may say results as near perfection as possible—were now obtained, but only at the cost of an alarming loss of platinum, which took place during ignition. A portion of this was recovered by breaking up the crucible—a rather porous one—and digesting in soda solution.

A fourth lot was attempted in a first-class Berlin porcelain crucible, which certainly is not open to the charge of porosity. Here, at the first glance, the interior of the crucible was seen to have been freely acted upon, although the yellow particles of precipitate were not all gone. Gelatinous silica was filtered abundantly from the evaporated and re-dissolved chloride, and the results obtained with pure potassium chloride were 100·45 and 100·38 per cent., using 6 drachms water and Stas's equivalents.

Now, these are only a portion of the experiments that were made in duplicate, and with the one object in view—namely, the determination of the best method of reduction and purification; for thus early in the

investigation the uniformly high results caused us to suspect other sources of error. The purity of the alcohol, both ethylic and methylated, the proper degree of concentration and temperature of solution of potassium salt when being precipitated, the temperature of the water bath during evaporation, the extent to which the evaporation was carried, the cooling and the washing, were all made the subjects of examination by potassium estimations, chiefly on pure potassium chloride, in order to discover possible sources of error; and as we proceeded with the experiments bearing on the main object of the investigation, we thus were assured that the high results were not caused by these secondary and independent sources of error. The absence of ammoniacal fumes and the adjustment of balances and weights were also scrupulously cared for.

We found that good results were to be obtained by the fourth method of recovering, with the ignition of the platinum in potassium nitrate; but we have seen, as has already been detailed, that these results were at the best uncertain and costly, and that sometimes the process was a failure, as far as complete reduction was concerned.

Reduction by ignition in presence of oxalic acid was tried, but found to be tedious when operating on a considerable quantity of dried washings and precipitates. Reduction by ignition in a current of hydrogen we have not tried. The investigation of this and other points connected with the determination of potassium we have not been able to proceed with, on account of pressure of business, and other circumstances over which we have no control.

For the reasons already enumerated, we returned to our usual method, which is, as previously stated, a modification of the second, or reduction by solution of sodium hydrate and methylated alcohol. This method, which embraces the drying and ignition of the platinum black alone, between the purifications with hydrochloric and with nitric acid, together with several precautions and refinements of manipulation during the estimation of the potassium, and the use of a correct factor for calculating, gives results which are constant and accurate, and satisfactory alike to science and to commerce.

This routine, although it requires a little attention to details, is neither so intricate nor so difficult as the liability to errors in platinum preparation and potassium estimations would seem to indicate. Applying this method to even very impure platinum—for instance, that recovered from the débris of a pounded crucible—we had, by the trial with pure potassium chloride, the following results,—100·08 and 100·00 per cent. We are far from desiring it to be supposed, however, that our manipulation is capable of that degree of accuracy in general.

Briefly, we have large excess of soda solution to platinum washings and precipitates diluted; boiling; addition of methylated spirit beyond that contained in the washings; sharp boiling, when reduction takes place readily; decanting, washing without or with the addition of sulphuric acid, according as the fluid settles clear or otherwise; boiling with 1 ounce hydrochloric acid to ½ ounce platinum black; washing and boiling till free from chlorine, which the boiling appears to liberate; drying and igniting,—the latter easily and quickly effected in common porcelain crucible over gas or good spirit-lamp, after which opportunity is offered to weigh the metal as a guide for the strength of the future solution; boiling in 1 ounce pure nitric acid; and ample washing with hot water, till free from acid re-action. The metal may then be dissolved in large excess of hydrochloric acid, with addition of small quantities of nitric acid as required; or it may be dissolved at once in 3 volumes hydrochloric acid, and 1 volume nitric acid, evaporated twice with dilute hydrochloric acid—say 1·1 specific gravity, to prevent effervescence—and finally with water. As much water is then added as will yield a solution containing 27 grains of metallic platinum per ounce, and the fluid filtered. From 6 to 7 drachms of this solution will be sufficient for 10 grains of the potassium salt.

The experiments quoted are only a few out of very many that we have made, with every care, bearing upon this subject, nearly every experiment having been done in duplicate.

Before leaving the first part of our subject, and taking up the consideration of the analysis of commercial potassium salts, it will only be proper first to refer to the interference of potassium sulphate with the accurate determination of potassium in commercial "muriates." It is a well-known fact that most of the muriate at present being made from kelp is particularly free from salts of sodium, but contains potassium sulphate, in proportion varying from 3 to 6 per cent. Now, potassium in the sulphate cannot be accurately estimated without the adoption of some means of removing the free sulphuric acid which is necessarily formed during the process of precipitation. With a view to this, some authorities recommend the conversion of the sulphate into chloride, by double decomposition with barium chloride; while others advocate the addition of an equivalent of pure sodium chloride. The latter course we have found highly satisfactory; and in the case of muriates containing 4 per cent. of sulphate, have always obtained too high results, whatever else was done, if the addition of the sodium chloride was omitted, the error averaging from ·3 to ·37 per cent.

In analyses of "plate sulphates," or "soft sulphates," or other salts containing potassium sulphate, without the certainty of more than a

sufficient quantity of sodium chloride being present than would take up all the liberated sulphuric acid, we invariably add this necessary excess of pure sodium chloride, and find that it contributes materially to constant and accurate results. We need only remind the analyst that free acids act on potassium chloroplatinate, (see Fresenius), and that free sulphuric acid is not expelled in the process, while free hydrochloric acid or free nitric acid in presence of free hydrochloric, is readily so; and that any sodium sulphate formed or present is easily removed by a proper system of washing the potassium precipitate.

With regard to the circumstance already mentioned, of too high results being obtained when precipitation is effected in a too concentrated solution, we have to state that this effect is produced even in the case of pure potassium salts. We are not quite certain that we can offer a clear explanation of this source of error; it is sufficient to say that, in many cases of precipitation of substances in strong solutions, impurities, if these be present, are carried down by the precipitate, and frequently obstinately resist washing with hot water,—as, for instance, alkaline salts carried down with barium sulphate (see Fresenius and other authorities). Even when no impurities are present, notable quantities of the precipitant are taken up and held firmly; and this, we believe, takes place when precipitating concentrated solutions of potassium salts with solution of pure platinic chloride. We therefore precipitate in the cold, and at first dilute, solution—using 6 drachms water to 10 grains salt—having found that attention to these points contributes to homogeneous, larger, more perfect, and isolated crystals of potassium chloroplatinate than rapid precipitation. The latter gives more confused, sometimes tufted and radiated, crystals. Both form interesting objects under the microscope, the former having all the polish and beauty of precious stones.

II. *The Manipulation of the Process.*

Next to the chemical principles involved, the manipulation of the analysis demands our most severe attention. We find that many chemists still adhere to the plan of weighing out a quantity of the salt, and drying at about 250° to 280° Fahr., powdering the dried salt, and after re-heating, to ensure thorough drying, weighing out a quantity—approximately 10 grains of the dried salt—which are then dissolved in water and precipitated by platinum chloride solution. We object to this method, as giving inconstant results, and that for the following reasons:—

1. The error of the balance is multiplied by a high figure in calculating the results to per cent. If the quantity weighed out be

$\frac{1}{100}$th of a grain in error, we have the result, from this cause alone, $\frac{1}{10}$th of a per cent. wrong, even if every other step in the process is absolutely perfect; and if the quantity weighed out be from the errors of the balance, the absorption of hygrometric moisture by the dried powder, and the loss by transference, $\frac{1}{10}$th of a grain in error, out comes the alarming result of 1 per cent. wrong.

2. The whole of the insoluble matter is necessarily included in the weight of the potassium chloroplatinate, or what is still worse, the small quantity must be dissolved and filtered, and the solution evaporated—a most tedious and unsatisfactory course of procedure.

3. If the dried powder be weighed between watch glasses that have been previously wiped dry, spurting of the powder sometimes takes place from electrical repulsion. This may be observed readily in the case of balances whose pans are suspended by silk cords. Some experiments showed that the average quantity of salt lost in this way was about ·02 grain, even when every precaution was observed to avoid loss. Nor should it be overlooked that finely powdered and thoroughly dry salts are highly hygrometric. Another objection is the difficulty of taking a perfectly fair sample of 10 grains from the dried and powdered quantity; and even the powdering causes loss.

Numerous experiments might be quoted to show that the results obtained by weighing dried salts are inconstant, or, at the best, give a somewhat constant error.

The plan we seek to recommend is that (advised by Fresenius and some of the other leading authorities on analysis) of dissolving a large quantity—say 500 grains of the salt—in a small quantity of water, and filtering the solution into a flask which is capable of containing, up to a fine mark on the narrow neck, exactly ten times as much water, weighed at 60° Fahr., as the salt employed; and after thoroughly mixing the filtrate and washings, and carefully sighting and adjusting with a fine pipette to the mark, drawing off, by means of a graduated pipette, an aliquot part of the solution for precipitation. The pipette for this purpose should be graduated to deliver exactly as much of the solution as will correspond to 10 grains of the original salt—in short, a 100-grain pipette; and it should be graduated to deliver, not by pressing or blowing out the last drop that adheres to the interior point of the instrument, but by touching two or three times the surface of the liquid that has been discharged from it, after having drained for ten seconds. A constant quantity thus remains behind, provided that the exterior point of the instrument, to the extent of $\frac{3}{8}$ths of an inch, receive a film of grease. A pipette constructed thus is capable of delivering to ·02 grain of liquid each time, which

represents only ·002 grain of the original salt; and we have proved by repeated trials that the greatest mean error of delivery in any two experiments does not exceed ·03 per cent. of the salt.

It may be well, however, to notice one or two objections that have been taken to this mode of operating volumetrically, by persons ignorant of the practical advantages of pipette measurement. It has been urged that if much potassium sulphate be present in the salt operated on, a portion of it is apt to be left insoluble on the filter. In answer to this, we simply say that the objection is ridiculous, the sulphate being *perfectly* dissolved, and with the greatest ease in the case of "muriates," when using a flask of 5,000 grains capacity, and 500 grains of the salt. When operating with sulphates we certainly prefer a solution of half this strength; but use a 200-grain pipette, and this mode of working may even contribute to greater accuracy.

It has also been objected that a pipette delivering a definite quantity of water will not deliver an exactly corresponding quantity of any saline solution, from the fact of the difference in density. While admitting that a pipette delivers a sensibly greater volume of saline solution than of water, we observe that the difference is but slight, and can be perfectly compensated for by adjusting the pipette to deliver *not water*, but the *salt solution*. In testing the pipette it is to be observed that it delivers *exactly* a decimal part of the solution at the same temperature, the trials being made by weighing the quantity of liquid delivered. The volume of the fluid delivered will be found to vary sensibly, according to the nature of the salt dissolved. This is a very nice point; but we have even used different pipettes for different kinds of solutions.

We adjust our pipettes by weighing off an accurate specific gravity flask of the solution of salt, and taking an aliquot part of the *weight* as the data for adjusting the pipette by repeated trials. In our case the specific gravity flask held 1061·3 grains; consequently the pipette was made to deliver 106·13 grains. Taking a pipette * that has been in use four years, we requested an assistant to weigh out 5 volumes of distilled water from it. This we did to ascertain, from curiosity, what results another person would obtain. We give the quantities:—

99·90, 99·97, 99·91, 99·93, and 99·93 grains,

which illustrates the whole question of pipette measurements. He finds ·035 grain of solution above and below the average. This is equal to ·0035 grain of the salt. Results obtained by ourselves showed that the

* Purchased as delivering 100 grains of water; but the mark was found to be $\frac{1}{16}$th inch in error.

pipette, which delivered 99·97 volumes of water, delivered 100 volumes potassium chloride solution.

III. *The Calculation of Results.*

It is quite obvious that although we master the method of obtaining pure platinum tetrachloride, and of manipulating the analysis of a potassium salt even to perfection, that the use of wrong equivalents or incorrect factors for calculating results must necessarily lead to error. From the fact that the combining numbers of the elements composing the potassium chloroplatinate have been made the subject of investigation by chemists of celebrity, we might well suppose that an error of calculation could scarcely take place; but when we reflect that the results obtained by some investigators materially differs from those of others, we perceive that, to say the least, some discretion is necessary in selecting for use in practice those that are most reliable and correct. We have also found that, among some chemists of standing and experience in practical analysis, factors for calculating potassium results are in use which are not only not based on unquestionable experiments, but would give results from ·5 to ·75 per cent. too high with pure potassium chloride, even if every other step in the process were absolutely correct.

The following are the combining numbers of the elements of potassium chloroplatinate, as obtained by different authorities:—

Stas, 1860,	39·130	Potassium.
Stas, 1865,	39·137	
Pelouse,	39·140	
Berzelius,	39·146	
Stas, 1860,	35·460	Chlorine.
Stas, 1865,	35·457	
Berzelius,	35·460	
Calculated from Stas's (1860) equivalent of Potassium and Chlorine, and subjoined data of Berzelius,	197·1836	Platinum.
Do. do., using Stas's equivalent obtained in 1865,	197·1937	

With regard to the determinations of the equivalents of potassium and chlorine, there cannot be any difference of opinion as to the most reliable numbers being those of Stas, the experiments by which they were obtained having been conducted with the highest degree of refinement of which modern science is capable. We therefore accept

them as truth, and as being the only numbers that give correct results in practice.

Stas has not published any account of experiments made with a view of arriving at the true equivalent of platinum. In the absence of such data we consider the equivalent of that element, as stated above (197·1937), to be the most reliable, some other numbers, as given by certain authorities, having been calculated from a questionable equivalent of potassium chloride. The result of Berzelius, obtained by igniting potassium chloroplatinate, and by the aid of which this equivalent of platinum has been arrived at, is described in Watt's *Dictionary*, vol. iv., p. 670, and is as follows:—

6·981 grammes 2 KCl, $PtCl_4$ lost by ignition, .	2·024 Cl.
And left a mixture of	2·822 Pt.
And	2·135 KCl.
Total, . .	6·981 grammes.

Watt then calculates—

2 KCl.		Pt.		2 KCl.		Pt.
2·135	:	2·822	: :	149·2	:	197·2094

This number for platinum approaches the truth very nearly, but is evidently based on K 39·1 and Cl. 35·5.

Making a similar calculation with the use of Berzelius's own numbers for K and Cl, we have—

2 KCl.		Pt.		2 KCl.		Pt.
2·135	:	2·822	: :	149·212	:	197·2254

Miller, by a like calculation, deduces for platinum the equivalent 197·12; the number used for potassium chloride is not stated. Assuming, however, that the numbers used are those given in the tables of his work (K 39·11 + Cl 35·46 = 2 KCl 149·14), we have—

2 KCl.		Pt.		2 KCl.		Pt.
2·135	:	2·822	: :	149·14	:	197·13

Calculating in a similar way, but using Stas's (1865) numbers, for K and Cl, we have—

2 KCl.		Pt.		2 KCl.		Pt.
2·135	:	2·822	: :	149·188	:	197·1937

The last number is undoubtedly derived from the very best data we

have. The following therefore must be regarded as the true equivalent of potassium chloroplatinate:—

			in 2 KCl. Pt. Cl_4.
Potassium,	.	39·1370 2 equivalents,	. 78·2740
Chlorine,	.	35·4570 6 „	. 212·7420
Platinum,	.	197·1937 1 „	. 197·1937
		Equivalent of Potassium Chloroplatinate,	488·2097

These figures yield the following factors:—

2 KCl, $PtCl_4$ to 2 K. Factor.

488·2097 ÷ 78·274 = ·16032866, or in practice ·1603

2 KClPtCl$_4$ to K_2O.

488·2097 ÷ 94·274 = ·19310145 „ ·193

2 KClPtCl$_4$ to 2 KCl.

488·2097 ÷ 149·188 = ·30558180 „ ·3056

These results make it sufficiently obvious that the factor ·193 must be used in calculating potassium chloroplatinate to potassium oxide, and that no other will give a true result. The number ·194, we find, was recently, and is still, used by some chemists, but is quite inadmissible, not being based on accurate and trustworthy experiments, and giving results equal to more than ·5 per cent. too high in practice. It has been defended on the ground of the solubility of the potassium chloroplatinate in platinum tetrachloride solution and in alcohol, but the truth is that this solubility, in the circumstances of a potassium determination is practically *nil*, and most distinctly no allowance should be made for it.

The use of a factor for calculating potassium or potassium oxide to potassium chloride, based upon an obsolete and long since acknowledged incorrect equivalent of potassium (39·0), we find to be still extensively used, and must condemn, as giving an additional and independent error of ·1 per cent. potassium chloride too high.

As the result of all our experiments in connection with potassium determinations, and of observations made in the manufacture of potassium salts on the large scale—a branch of chemical industry in which one of us has been engaged for many years—we submit the following conclusions:—

1. That the methods of analyzing potassium salts, as practised and taught in some laboratories, are very imperfect.

2. That reliance in a so-called "full analysis," as a check on a potassium determination is delusive, and is, within a few per cent. of potassium chloride, totally valueless.

3. That any number of mere determinations of potassium, however closely they may agree, is no guarantee of accuracy, the error being to a large extent a constant one.

4. That the use of the factor ·194 for the conversion of potassium chloroplatinate into potassium oxide, or indeed the use of any other factors than those based upon Stas's equivalents, is erroneous, such not being deduced from reliable experiments.

5. That it is necessary to check the process used, and to be satisfied of the purity of the re-agents employed, and of the absence of other disturbing causes, by experiments with pure potassium chloride or other potassium salt; and that in no case should results be reported unless controlled by such experiments.

MINUTES.

Anderson's University Buildings, November 6, 1867.

The Sixty-sixth Session of the Philosophical Society of Glasgow was opened this evening, Dr. Francis H. Thomson, the President, in the Chair.

Dr. Theodore Merz was elected a member.

Mr. William Ramsay and Mr. William Johnston were appointed Auditors of the Treasurer's Accounts.

The President reported on the state of the negotiations for obtaining accommodation for the Society and its Library in the buildings of the Corporation Gallery. In connection with this subject, he mentioned that a report had been prepared last Session by a Committee of Council, embodying a proposal to the Trustees of the Fund accruing from the Exhibition of 1846 to allow the Society the use of the money on certain conditions favourable to the public. Reasons were suggested to the Council for refraining from giving publicity at that time to the report; but as these reasons no longer existed, he would now, with the permission of the Society, call upon Mr Crosskey to read the report.

The report was thereupon read; and after some remarks by Mr. George Anderson, Mr. John Ramsay, Mr. Provan, Mr. Taylor, and Dr. Allen Thomson, it was agreed that the further consideration of the subject be deferred till next meeting, and that in the interim the report be printed and circulated amongst the members, marked "Proof for use of Members only."

The President then delivered an opening address, noticing at the outset the loss which the Society had sustained, since it last met, by the death of Mr. Walter Crum. The President described, from personal observation, the extensive Jute Works of Messrs. Cox Brothers, near Dundee; he next made some observations on mineral and manufactured oils, and on their hydro-carbons as applicable to heating and lighting purposes; concluding with an account of recent improvements in the manufacture of iron and steel.

Dr. Allen Thomson referred to the obligations under which the

President had laid the Society by his persevering efforts for its better accommodation; and after remarking on the judicious choice and treatment of the topics contained in the address, moved that a vote of thanks be given to Dr. Thomson, which was cordially agreed to.

November 20, 1867.—The annual meeting of the Philosophical Society, for the election of Office-bearers and other business, was held this evening, Dr. Francis H. Thomson, the President, in the Chair.

The following were elected members, viz.:—

Mr. James Mactear, St. Rollox Chemical Works; Mr. Joseph Agnew, Surgeon Dentist, 47 Bath Street; Dr. George Robertson, 39 Bath Street; Mr. William Johnston, 21 Virginia Street; Mr. John Johnston, 52 West Howard Street; Mr. William M'Onie, Scotland Street Engineer Works.

The Secretary read the following Report on the State of the Society, which was approved of, and ordered to be printed in the *Proceedings*:—

Report by the Council on the State of the Society.

I. *The Proceedings.*—The printed *Proceedings* of the Society in Session 1866-67 extended to 95 pages. A review of the rise and progress of some of the principal workshops of Glasgow formed the substance of the President's address at the opening of the Session, showing the gradual development of the iron trade, of iron shipbuilding, and the construction of locomotives. Dr. R. Angus Smith, of Manchester, communicated the results of numerous Analyses of the Water of the River and Frith of Clyde, which gave rise to an interesting discussion, reported in the *Proceedings.* This was followed by a paper, "On the Scientific Premonitions of the Ancients," by Professor Young, tracing the indications of geological knowledge amongst the Greeks. Mr. John D. Campbell brought under the notice of the Society "The Proposed Extension of the Factory Act, and its Probable Effects on Juvenile Labour and Education in Glasgow." The discussion which followed afforded several of the members occasion for expressing their opinions in favour of a system of national education. Professor W. J. Macquorn Rankine furnished a paper "On the Phrase *Potential Energy*, and on the Definitions of Physical Quantities," being a reply to certain strictures by Sir John Herschel. Professor Young

described two Instruments contrived for the exact Measurement of the Proportions of Fishes and Shells, so as to determine the limits of their variation within their own areas. Professor Sir William Thomson delivered a discourse "On Vortex Atoms," illustrated by experiments, and kindly promised to continue the subject on a future occasion. Mr. A. S. Herschel described to the Society the phenomena of the Shower of Meteors on the morning of the 14th of November, 1866, Mr. William Gorman supplied the "Results obtained from Heating Scrap Iron in the Heat-restoring Gas Furnace, and in the Furnaces commonly employed for that purpose, with Comparisons and Observations on the Quality of the Welded Iron, and on the Flame of the Common and Gas Furnaces." The Session was closed by an account, by Dr. Allen Thomson, of "Recent Discoveries in the Structure of the Internal Ear."

The following additional communications were not printed in the *Proceedings*, viz.:—Mr. A. S. Herschel, "On Singing Flames, and the Musical Sounds produced by Heat." Description by Sir William Thomson of "The Marine Galvanometer, and other Apparatus for Electrical Tests, used on board the *Great Eastern* in the recent Expedition." Professor Young, "On the Bones of the Face in Vertebrates." Notice by Dr. Bryce of "Some New Discoveries in the Geology of Arran." Mr. Mayer exhibited and described the action of Ansell's Indicators of Fire Damp in Coal Mines. He gave an account of the method of extracting the metal Magnesium, with some of its applications; and also exhibited and described Gale's protected Gunpowder, and Nitro-glycerine, or Blasting Oil.

II. *Number of Members.*—The Treasurer reports that at the commencement of last Session the number of members was 282. The number was increased by 17 in the course of the Session, making 299 in all. This number has been diminished by arrears, resignations, removals, and deaths, to 278, of whom 7 owe one year's, 3 owe two years', and 1 three years' subscriptions. A notable proportion of the resignations arise from the inconvenience of the present place of meeting.

III. *Accommodation of the Society.*—The Council bestowed much attention on this subject during the currency of the Session; and the President was in frequent communication with the Lord Provost, in the course of the past summer, with a view to the maturing of a plan which might embrace the twofold object of obtaining suitable accommodation for the Society, and a satisfactory settlement of the Society's claim upon the money accruing from the Exhibition Fund of 1846. The Council's report placed in the hands of the members explains

the nature of the proposal which has been made to the Trustees on the fund, and the Council will be guided in their future proceedings by the decision of the Society this evening.

Rev. Mr. Crosskey, the Librarian, read a report on the state of the Library, which was approved of.

Mr. Mann, the Treasurer, gave in the following Abstract of his Account for Session 1866-67, which was approved of:—

Dr.

1866.—*Nov.* 1.						
To Cash in Union Bank of Scotland,	£4	8	9			
,, Cash in Treasurer's hands,	3	13	3			
				£8	2	0
1867.—*Oct.* 31.						
,, Entry Money and Dues from 1 new Member, 1865-66,	2	2	0			
,, Entry Money and Dues from 15 new Members, 1866-67, at 42*s*.,	31	10	0			
,, Annual Dues from 6 Original Members at 5*s*.,	1	10	0			
,, Do. from 250 Members at 21*s*.,	262	10	0			
,, Do. from 4 Members for two years,	8	8	0			
				306	0	0
,, Institution of Engineers for Rent,				15	0	0
,, Interest on Bank Account,				0	15	8
,, Taxes recovered from Landlord,				2	12	10½
				£332	10	6½

Cr.

1867.—*Oct.* 31.						
By New Books and Binding,				£91	9	2
,, Printing *Proceedings*, Circulars, &c.,				34	7	9
,, Stationery,				1	12	0
,, Salaries and Wages,	£110	0	0			
,, Delivering and Posting Circulars,	15	12	3			
,, Rent, Insurance, Gas, Coals, and Water,	57	15	4			
,, Taxes, Subscription to Ray, Palæontographical, and Cavendish Societies, and Petty Charges,	13	18	9			
				197	6	4
,, Balance—In Union Bank of Scotland,	3	2	7			
,, Treasurer's hands,	4	12	8½			
				7	15	3½
				£332	10	6½

The Society proceeded to the Sixty-sixth Annual Election of Office-bearers, who were appointed as follows, viz.:—

President.

DR. FRANCIS H. THOMSON.

Vice-Presidents.

JAMES BRYCE, M.A., LL.D., F.G.S.

PROFESSOR ROBERT GRANT, M.A., LL.D., F.R.S.

Librarian.

REV. HENRY W. CROSSKEY, F.G.S.

Treasurer.

MR. JOHN MANN, C.A.

Secretary.

MR. WILLIAM KEDDIE, F.R.S.E.

Other Members of Council.

MR. DANIEL MACNEE.

MR. JOHN BURNET.

PROFESSOR THOMAS ANDERSON, M.D., F.R.S.

MR. ALEXANDER HARVEY.

MR. JOHN RAMSAY, Lord Dean of Guild, M.P.

PROFESSOR W. J. MACQUORN RANKINE, LL.D., F.R.S.

PROFESSOR ALLEN THOMSON, M.D., F.R.S.

SIR WILLIAM THOMSON, LL.D., F.R.S.

MR. WILLIAM RAMSAY.

MR. GEORGE ANDERSON.

MR. ALEX. S. HERSCHEL, B.A.

PROFESSOR JOHN YOUNG, M.D., F.R.S.E.

The Society then took into consideration the Report of the Council on the Exhibition Fund. After discussion, it was agreed that the terms proposed by the Society to the Trustees should be limited to the offer "to give permanent facilities (on such a scale as the accommodation which can be provided by the limited fund may permit) for the exhibition of models and specimens," without the proposed addition of granting a limited number of free reading tickets entitling students to consult the books in the Library.

The following were appointed a Deputation to bring the subject formally before the Trustees, viz.:—

Dr. Allen Thomson; Mr. Ramsay, Lord Dean of Guild; Rev. Mr. Crosskey; Mr. David More.

Dr. Thomson, the President, and Dr. Bryce, senior Vice-President, are the Trustees for the Society; the Lord Provost and the senior Bailie are the Trustees for the Town Council.

December 4, 1867.—*The* President *in the Chair.*

Dr. George R. Mather, 11 Annfield Place, and Mr. Robert Rowland Stephen, Adelphi Biscuit Factory, were elected members.

The following gentlemen were nominated for election:—

Dr. Cowan, Professor of Materia Medica in the University of Glasgow; Mr. James Church, Merchant, 88 Renfield Street; Mr. James Maclehose, Bookseller, Victoria Crescent; Mr. D. C. Watson, Bookseller, 70 St. Vincent Street; Mr. Daniel Munro, House Factor, 16 Abbotsford Place.

In consideration of the approach of the Christmas recess, the Society agreed to ballot for the election of these gentlemen at the present meeting, and they were elected accordingly.

The Secretary read a minute of a special meeting of Council, held on the 29th of November, to receive the report of the deputation appointed at last meeting of the Society to communicate with the Trustees on the Exhibition Fund. After hearing their report, the Council framed two resolutions, which they agreed to recommend to the Society as a basis for the settlement of its claim upon the fund; which resolutions the Secretary was instructed to print in the circular convening the next meeting of the Society. The resolutions are as follows:—

"I. That the Council be empowered to make the following proposals to the Trustees of the Exhibition Fund:—

"(1.) On condition that the interest of the money now accumulated be paid to the Society, the Philosophical Society will agree to place at the disposal of the Trustees of the Fund (namely, the Lord Provost and senior Bailie, and the President and senior Vice-President of the Society) a limited number of tickets for enabling readers to consult their books within the Society's premises.

"(2.) If sufficient accommodation, in addition to the Library, can be provided by the fund placed at the disposal of the Society, the Philosophical Society will also agree to take charge of the exhibition of patented and other inventions (models of machinery, or otherwise), new industrial products, &c., &c., to which access shall be given on the most liberal terms.

"II. That the Society agree to remove to the Corporation Galleries, on condition that the Trustees of the Exhibition Fund pay the interest of that fund to the Society; and provided that suitable accommodation be afforded at a rental not exceeding £100 a year."

The adoption of the resolutions was moved by Mr. Ramsay, Lord Dean of Guild, who explained to the Society that the Council were

shut up to the necessity of restoring the proposal to grant tickets for consulting the Library, by the position assumed by the Trustees for the Town Council.

The motion was seconded and supported by Dr. Allen Thomson, and after some discussion, the resolutions were passed, with some verbal alterations.

Professor Thomas Anderson, M.D., made a communication "On the early History of the Distillation of Coal and other Bituminous Substances."

December 18, 1867.—*The* PRESIDENT *in the Chair.*

The following gentlemen were nominated as members; and in consideration of the adjournment of the Society during the holidays, it was agreed to proceed to ballot for their election this evening, and they were elected accordingly, viz.:—

Mr. Duncan M'Gregor, Optician, 39 Clyde Place; Mr. J. P. Smith, C. E., 67 Renfield Street; Mr. R. J. Currie, Merchant, 115 St. Vincent Street.

The Society, on the suggestion of the Council, resolved to adjourn over the holidays till the 22nd of January.

Mr. Alexander S. Herschel, B.A., delivered a discourse "On Vibrating Strings in connection with Musical Harmony."

January 22, 1868.—*The* PRESIDENT *in the Chair.*

The PRESIDENT stated that his negotiations with the Lord Provost on the subject of the accommodation of the Society in the buildings of the Corporation Gallery were making satisfactory progress, and that he hoped to be in circumstances to announce a definite result on an early occasion.

MR. CROSSKEY, in reporting on the addition of new books to the Library, mentioned the presentation by the President of Mr. J. Scott Russell's work *On the Modern System of Naval Architecture*, in three folio volumes. The thanks of the Society were given to the President for his gift.

PROFESSOR GRANT made a communication "On the Planet Jupiter seen without his Satellites;" and subsequently explained an arrangement at the Glasgow Observatory, with Government, for taking part in a general system of storm signals, founded on observations made in different parts of the United Kingdom, and in France and Spain.

February 5, 1868.—*The* PRESIDENT *in the Chair.*

A letter from Mr. Monro, Town Clerk, was read by the PRESIDENT, requesting that the Society would make an offer in writing for accommodation in the Corporation Buildings. The President received the authority of the Society to make a written offer, in terms of the resolution of the Society on the 4th December, 1867, namely:—"That the Society agree to remove to the Corporation Galleries, on condition that the Trustees of the Exhibition Fund pay the Interest of that Fund to the Society; and provided that suitable accommodation be afforded at a rent not exceeding £100 a year."

DR. BRYCE gave an "Account of Recent Researches respecting the Internal Heat of the Globe."

February 19, 1868.—*The* PRESIDENT *in the Chair.*

Mr. William Schofield, 335 Sauchiehall Street, was elected a member.

MR. ST. JOHN VINCENT DAY read a paper "On certain Theories concerning the Purpose and Primal Condition of the Great Pyramid of Jeezeh."

March 4, 1868.—DR. BRYCE, *Vice-President, in the Chair.*

MR. STANFORD reported that a circular inviting members of the Society interested in Chemistry to constitute themselves into a Chemical Section had elicited a general and favourable response. The circular contained the following conditions proposed by the Council, in accordance with the former practice of the Society, for the formation of a Chemical Section, viz.:—

1. Room rent-free.
2. Right of consultation of Library given.
3. Members of the Society to be admitted to the meetings free. Others to be admitted as "Associates of the Section" on payment of an annual subscription of five shillings.
4. Section to elect its own officers.
5. Funds to be paid to the Society.

In order to give the Society's authority to the subscription of five shillings annually by Associates of the Section not members of the Society, DR. BRYCE intimated that he would bring a formal motion to that effect before the next meeting.

A paper "On the Correlation of Force in its bearing on Mind," by Dr. John Young, was read.

March 18, 1868.—*The* President *in the Chair.*

On the motion of Dr. Bryce, the Society authorized the annual payment of a subscription of five shillings by each of the associate members of the Chemical Section, not being members of the Society.

Sir William Thomson exhibited and explained new Electrical Instruments for delicate testing purposes, for use at sea.

Mr. Edmund Hunt read a paper "On a new Plastic Material," the invention of Mr. John Clark, Junior, Mile-End.

Dr. Francis H. Thomson read a paper "On the Fusibility of Trappean Rocks," and exhibited specimens having the vitreous appearance produced by sudden cooling.

April 1, 1868.—*The* President *in the Chair.*

Mr. St. John Vincent Day read a paper "On the Present State of some branches of Iron Metallurgy."

April 15, 1868.—*The* President *in the Chair.*

Mr. Charles Tennant, St. Rollox Chemical Works, Mr. William Galbraith, St. Rollox Chemical Works, and Mr. William Henderson, 180 West George Street, were elected members.

Mr. John Joseph Turnbull, 37 West George Street, and Mr. Robert R. Tatlock, F.C.S., 151 East George Street, were proposed as members; and in consideration of the approaching termination of the Session, they were elected this evening.

Dr. Allen Thomson made a communication "On the Brain of the Marmoset and other Simiæ."

Mr. MacTear gave in a report of the proceedings of the Chemical Section, and described a process for the recovery of Sulphur from Alkali Waste, as practised at St. Rollox Works, and which had been the subject of a paper read to the Section by Mr. Ludwig Mond.

April 29, 1868.—*The* PRESIDENT *in the Chair.*

The following papers were read, viz.:—

REV. H. W. CROSSKEY, "Geological Notes of a Journey in Norway, by Mr. Crosskey and Mr. David Robertson."

MR. JAMES MACTEAR, "On the Sources of Sulphur used in the Manufacture of Sulphuric Acid."

MR. JAMES THOMSON, "On the Difference of Structure in some Genera of Carboniferous Corals."

The PRESIDENT congratulated the Society on the close of another successful session. He announced that the arrangements for the accommodation of the Society in the Corporation Buildings in Sauchiehall Street were now completed; and mentioned that he had seen the plan for the new rooms, which would afford ample accommodation not only for the meetings of the Society, but for the Library, which had long been suffering from damp in the present building. They could not leave this time-honoured hall, however, without looking back with regret to the eminent men with whom they had once been associated—Professor Graham, and the late Dr. Thomas Thomson, Mr. Walter Crum, and others distinguished in science, and whose names would long be remembered in connection with the meetings of the Society in this place. The President trusted that the expense attending the removal of the Society and the furnishing of the new rooms would be provided for by a subscription amongst the members; and he hoped to have the pleasure, for the last time, of presiding at the opening of another session, and of welcoming the Society to the first meeting in their new hall.

DONATIONS TO THE LIBRARY,

FROM 1ST MAY 1867, TO 30TH APRIL 1868.

Æsthetic Culture. An Address delivered at the opening of the Fourteenth Session of the University Philosophical Society, Dublin, by G. Francis Armstrong. 8vo. Dublin, 1867. (From the Society.)

Amsterdam, Koninklijke Akademie van Wetenschappen, Verslagen en Mededeelingen der, Afd. Letterk. Vol. 10. 8vo. Amsterdam, 1866.

——————— Jaarboek, voor 1866. 8vo. Amsterdam, 1866.

——————— Processen Verbal, Afd. Letterk. 8vo. Amsterdam, 1867. (From the Academy.)

Anthropological Society of London, Review and Journal of the. Nos. 18, 19, 20, and 21. 8vo. London, 1867-68. (From the Society.)

Bath Natural History and Antiquarian Field Club, Proceedings of the. No. 1. 8vo. Bath, 1867. (From the Club.)

Belgique, Bulletins de l'Académie Royale de. Vols. 22, 23, and 24. Second Series. 8vo. Bruxelles, 1866-67.

———, Annuaire de l'Académie Royale de. 2 Vols. 18mo. Bruxelles, 1867-68. (From the Academy.)

Berlin, Monatsbericht der Kön. Preuss. Akademie der Wissenschaften Zu. April—December, 1867. 8vo. Berlin, 1866. (From the Academy.)

Berwickshire Naturalists' Club, Proceedings of the. Vol. 1, No. 5; Vol. 3; Vol. 4, 5 Nos.; and Vol. 5, 5 Nos. 8vo. Alnwick, 1837-67. (From the Club.)

Black (Joseph, M.D.), Lectures on the Elements of Chemistry, delivered in the University of Edinburgh. 2 Vols. 4to. Edinburgh, 1803. (From George Smith, Esq., 2 Shaftesbury Terrace, Glasgow.)

Bordeaux, Mémoires de la Société des Sciences Physiques et Naturelles de. Vols. 1-4, and Vol. 5, Parts 1 and 2. 8vo. Bordeaux, 1854-67. (From the Society.)

Boston Society of Natural History, Proceedings of the. Vol. 10, Sign, 19 to end. Vol. 11, Sign. 1-6. 8vo. Boston, 1866.

——————— Memoirs of the. Vol. 1, Parts 1 and 2. 4to. Boston, 1866-67.

——————— Annual Report of the. 8vo. Boston. 1866. (From the Society.)

Botanical Society of Edinburgh, Transactions of the. Vol. 9, Part 1. 8vo. Edinburgh, 1867. (From the Society.)

Bristol Naturalists' Society, Proceedings of the. Vol. 2, Nos. 9 and 10. 8vo. Bristol, 1867. (From the Society.)

Chemical Society, Journal of the. Vol. 5, New Series. May—December, 1867. Vol. 6, New Series. January—April, 1868. 8vo. London, 1867-68. (From the Society.)

Christiania, Forhandlinger Videnskabs-Selskabet i, Aar 1859-66. 8vo. Christiania, 1859-67. (From the University of Christiania.)

Day (St. John Vincent, C.E.), On Certain Points in the Manufacture of Malleable Iron, with special reference to the Richardson Process. 8vo. Glasgow, 1868. (From the Author.)

Directory (Art), with Regulations for promoting Instruction in Art. 8vo. London, 1866.

——— (Science), with Regulations for establishing and conducting Science Schools and Classes. Sixteenth Edition. 8vo. London, 1868. (From the Science and Art Department, South Kensington.)

Drummond (Thomas, R.E., F.R.A.S.), Memoir of, by John F. M'Lennan, M.A. 8vo. Edinburgh, 1867. (From P. M'Farlane, Esq., Comrie, Perthshire.)

Duncan (P. Martin, M.B.) and James Thomson, On Cyclophyllum, a new Genus of the Cyathophyllidæ, with Remarks on the Genus Aulophyllum. 8vo Pamphlet. No place or date. (From J. Thomson, Esq., Eglinton Street, Glasgow.)

Gehler's (Johann Samuel Traugott) Physikalisches Wörterbuch neu bearbeitet von Brandes, Gmelin, Horner, Littrow, Muncke, and Pfaff. 23 vols. Text, 8vo, and 2 vols. Plates, 4to. Leipzig, 1825-45. (From John Morgan, Esq., Springfield House, Bishopbriggs.)

Geological and Polytechnic Society of the West Riding of Yorkshire, Proceedings of the. Vol. 1, Parts 1, 3, and 6; Vol. 2; Vol. 3, Parts 1-7 and 9-10; and Vol. 4, Parts 1-8. 8vo. Leeds, 1840-68. (From the Society.)

Geological Society of Glasgow, Transactions of the. Vol. 1, Part 1, Second Edition, and Vol. 2. 8vo. Glasgow, 1864-68. (From the Society.)

Geological Survey of India, Memoirs of the. Vol. 5, Parts 2 and 3. 8vo. Calcutta, 1866.

——————— Palæontologia Indica. Series 3, Parts 10-13. 4to. Calcutta, 1866.

——————— Catalogue of the Organic Remains belonging to the Cephalopoda in the Museum of the. 8vo. Calcutta, 1866.

——————— Catalogue of the Meteorites in the Museum of the. 8vo. Calcutta, 1866.

——————— Annual Report of the, and of the Museum of Geology, Calcutta. 8vo. Calcutta, 1866. (From the Governor General of India.)

Guldberg (C. M.) and P. Waage. Études sur les Affinités Chimiques. 4to. Christiania, 1867. (From the University of Christiania.)

Herschel (A. S., B.A., F.R.A.S.), Meteors and Meteorites: a Discourse delivered before the British Association at Dundee. 12mo. Dundee, 1867. (From the Author.)

Historic Society of Lancashire and Cheshire. Transactions of the. Vol. 6. 8vo. Liverpool, 1866. (From the Society.)

Institution of Engineers in Scotland. Transactions of the. Vol. 10. 8vo. Glasgow, 1867. (From the Institution.)

Institution of Mechanical Engineers. Proceedings of the, from 1853 to 1866, and 3 Parts for 1867. 8vo. Birmingham, 1853-67. (From the Institution.)

Leeds Philosophical and Literary Society. Transactions of the. Vol. 1. Part 1. 8vo. London, 1837.

——— Annual Reports of the. 1824-28, 1830-31, 1833-39, 1840-42, 1844-48, 1849-51, 1852-59, 1860-64, and 1866-67. 8vo. Leeds, 1825-67.

——— Papers read before the, viz:—

Bickersteth (Right Rev. R.), The Physical Condition of the People in its bearing upon their Social and Moral Welfare. 8vo. Leeds, 1860.

Crawfurd (John, F.R.S.), China and its Trade. 8vo. London, 1858.

Hennessy (Prof. Henry, F.R.S.), The Relations of Science to Modern Civilization. 8vo. Leeds, 1862.

Herschel (Sir J. W., Bart.), Sensorial Vision. 8vo. Leeds, 1858.

Owen (Prof., F.R.S.), Inaugural Address on the opening of the New Philosophical Hall, Leeds. 8vo. Leeds, 1862. (From the Society.)

Mann (Alexander, M.A.), Remarks on the Administration and Defects of the Patent Laws, and the Inquiry of the Royal Commissioners. 8vo. London, 1867. (From the Author.)

Mond (Ludwig), On the Recovery of Sulphur from Alkali Waste. 8vo Pamphlet. Liverpool, 1868. (From the Author.)

M'Millan (John), Cause of Cattle Disease. 8vo. Castle-Douglas, 1867. (From James Napier, Esq., F.C.S.)

Napier (James), On some Popular Superstitions common in Partick forty years ago. 8vo. No Place or Date. (From the Author.)

Nautical Almanac for 1865 and 1866. 2 Vols. 8vo. London, 1861-62. (From Dr. W. G. Blackie, North Frederick Street, Glasgow.)

Portland Society of Natural History, Proceedings of the. Vol. 1, Part 1. 8vo. Portland, 1862.

——— Journal of the. Vol. 1, No. 1. 8vo. Portland, 1864. (From the Society.)

Quetelet (Ernest), Mémoire sur la Température de l'Air à Bruxelles. 4to. Bruxelles, 1867. (From M. Ad. Quetelet, Brussels.)

Rankine (W. J. Macquorn, C.E., LL.D., F.R.S.), On a Property of Curves which fulfil the Condition $\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0$. 8vo. 1867.

——— Some Remarks on Apparent Negative Slip. 4to. 1867.

——— On the Economy of Fuel, comprising Mineral Oils. 8vo. 1867.

Rankine (W. J. Macquorn, C.E., LL.D., F.R.S.), Mechanics (Applied), Reprinted from the Eighth Edition of the "Encyclopædia Britannica." 4to. No Date.

——— On Isoperimetric Regular Polygons. On the Approximate Rectification of Circular Arcs: being a Second Supplement to a Paper read to the British Association. 8vo. 1867.

——— On the Approximate Drawing of Circular Arcs of given Lengths. 8vo. 1867. (From the Author.)

Reale Istituto Lombardo, Rendiconti del. Classe di Scienze Matematiche e Naturali. Vol. 2, Parts 9 and 10; Vol. 3, Parts 1-9. 8vo. Milano, 1865-66.

——— Classe di Lettere e Scienze Morali e Politiche. Vol. 2, Parts 8-10; Vol. 3, Parts 1-10. 8vo. Milano, 1865-66.

——— Memorie del. Classe di Scienze Matematiche e Naturali. Vol. 10, 3 Parts. 4to. Milano, 1865-66.

——— Classe di Lettere e Scienze Morali e Politiche. Vol. 10, 4 Parts. 4to. Milano, 1865-66.

——— Annuario del. 12mo. Milano, 1866. (From the Institute.)

Report of the Commissioner of the U. S. Patent Office for 1863 and 1864. 4 Vols. 8vo. Washington, 1866. (From the Hon. the Commissioner of Patents, U. S.)

Report (Annual) of the U. S. Secretary of War. 8vo. Washington, 1866. (From Hon. E. M. Stanton.)

Royal Dublin Society, Journal of the. Nos. 1-19, and 22-36. 8vo. Dublin, 1856-67. (From the Society.)

Royal Institution of Great Britain, Proceedings of the. Vol. 5, Parts 1 and 2. 8vo. London, 1867. (From the Institution.)

Royal Society of Edinburgh, Proceedings of the. Nos. 71, 72, and 73. 8vo. Edinburgh, 1866-67. (From the Society.)

Russell (J. Scott, F.R.S.), The Modern System of Naval Architecture. 3 Vols. Folio. London, 1865. (From the President, Dr. F. H. Thomson.)

Scottish Meteorological Society, Journal of the. Nos. 4, 7-14, 16, and 17. New Series. 8vo. Edinburgh, 1864-68. (From the Society.)

Smithsonian Institution, Annual Report of the, for 1865. 8vo. Washington, 1866. (From the Institution.)

Society of Arts, Journal of the. Vol. 16, No. 797-806. 8vo. London, 1868. (From the Society.)

South Kensington Museum, A Guide to the Art Collections of the. 8vo. London, 1868. (From the Science and Art Department, South Kensington.)

Thomson (Sir Wm., LL.D., F.R.S.), On the Dynamical Theory of the Earth. 4to. 1854.

——— On the Rigidity of the Earth. 4to. 1862.

Thomson, (Sir Wm., LL.D., F.R.S.), On the The rmal Effects of Fluids in Motion. Parts 3 and 4. 4to. 1860-62.

——————————— On the Rate of a Clock or Chronometer, as influenced by the Mode of Suspension. 8vo. 1867.

——————————— On a Self-acting Apparatus for multiplying and maintaining Electric Charges, &c. 8vo. 1867.

——————————— On the Elasticity and Viscosity of Metals. 8vo. 1865.

——————————— On the Tidal Retardation of the Earth's Rotation. 8vo. 1866.

——————————— Note on the Physical Cause of the Submergence and Emergence of the Land during the Glacial Epoch. 8vo. 1866.

——————————— On the Convective Equilibrium of Temperature in the Atmosphere. 8vo. 1862. (From the Author.)

Tillman (S. D., A.M.), A new Chemical Nomenclature 8vo. Albany, 1866. (From the Author.)

Unger (C. R.), Morkinskinna. 8vo. Christiania, 1867. (From the University of Christiania.)

Watson (Wm. West, F.S.S.), Report upon the Vital, Social, and Economic Statistics of Glasgow for 1863, 1864, and 1866. 8vo. Glasgow, 1865-67. (From the Author.)

Wien, Mittheilungen der Kais. König. Geographischen Gesellschaft in. Vol. 9. 8vo. Wien, 1865. (From the Society.)

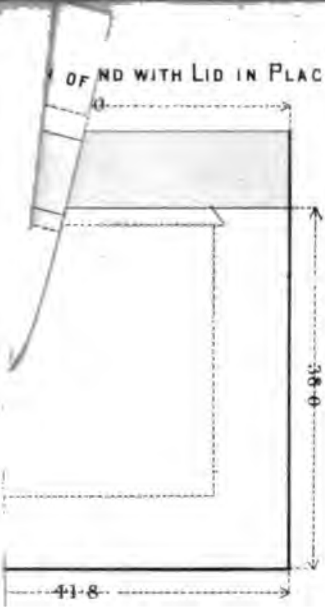
Woolhope Naturalists' Field Club, Transactions of the; including the first part of the Flora of Herefordshire (1866). 8vo. Hereford, 1867. (From the Club.)

INDEX TO VOL. VI.

BELL AND BAIN, PRINTERS, MITCHELL STREET, GLASGOW.

Philosophical Society's Proceedings, Glasgow 1868.

OF END WITH LID IN PLACE.



NSVERSE SECTION.

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N

SCALE

OF BRITISH INCHES.

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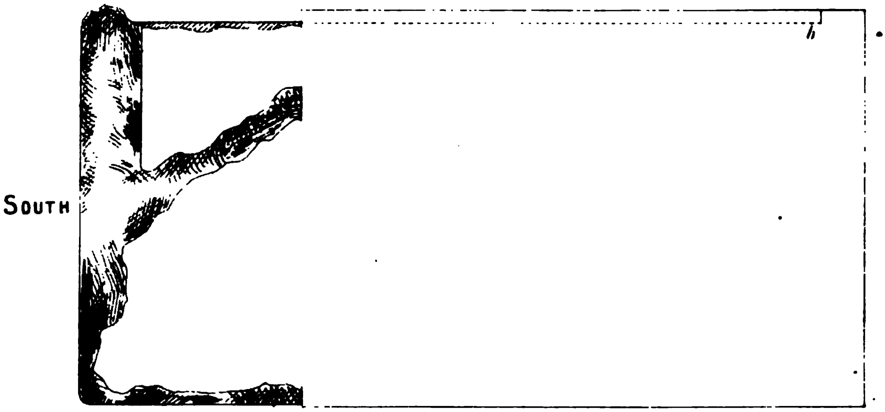

FIGS.

FIG 4

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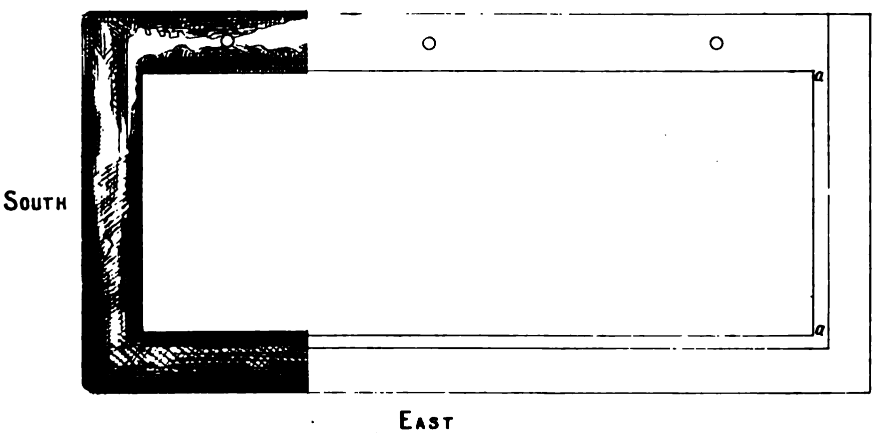
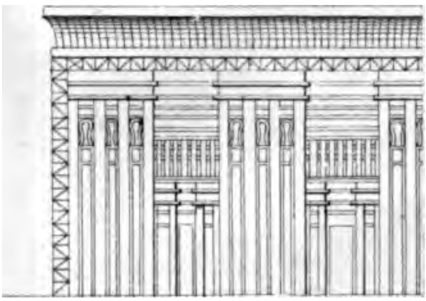
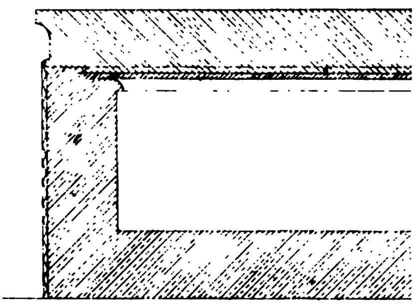
OGES AND FIXING PIN HOLES.





SIDE ELEVATION



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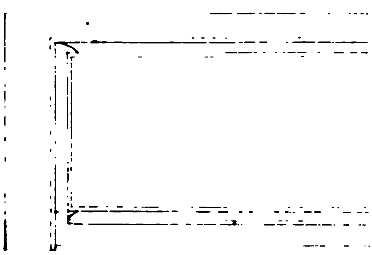

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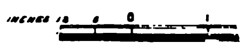
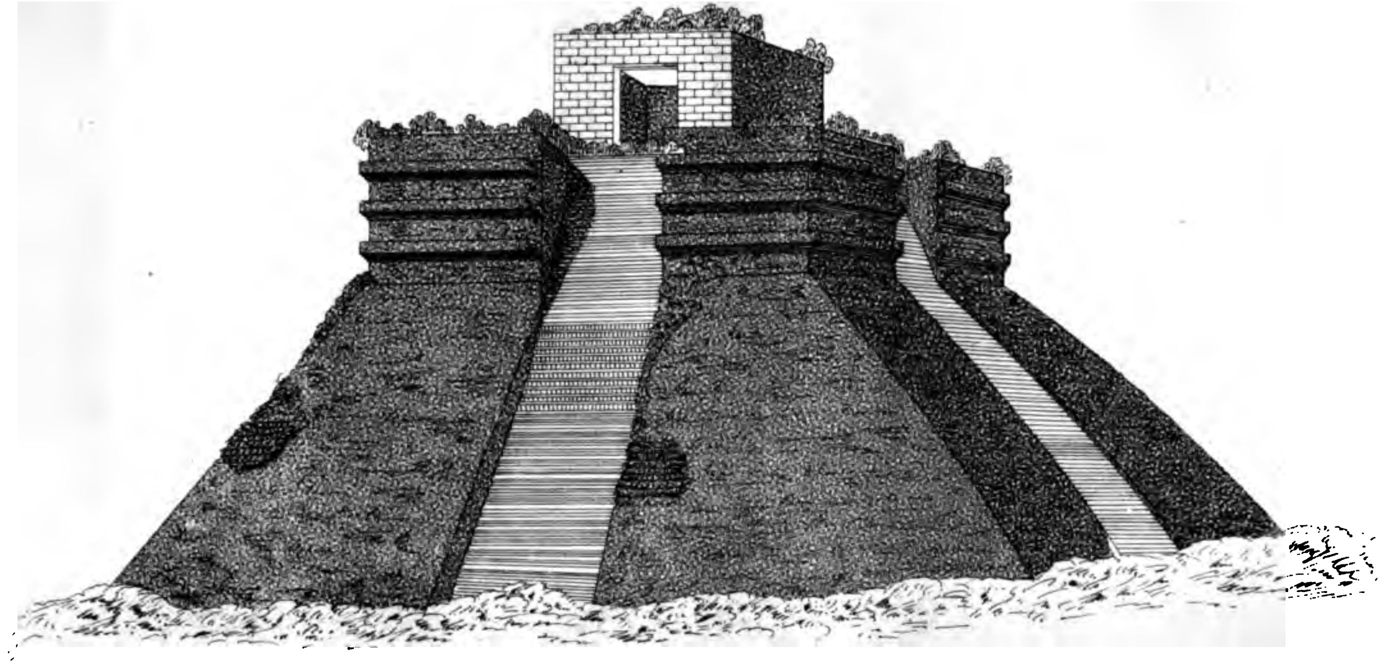
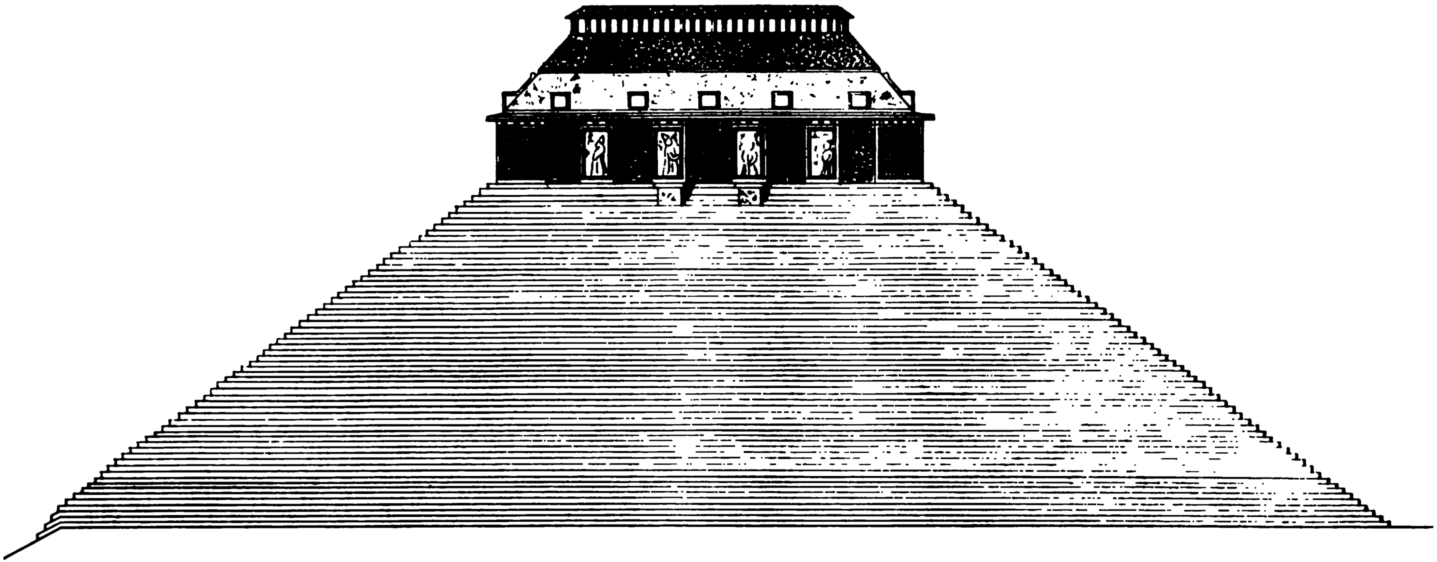


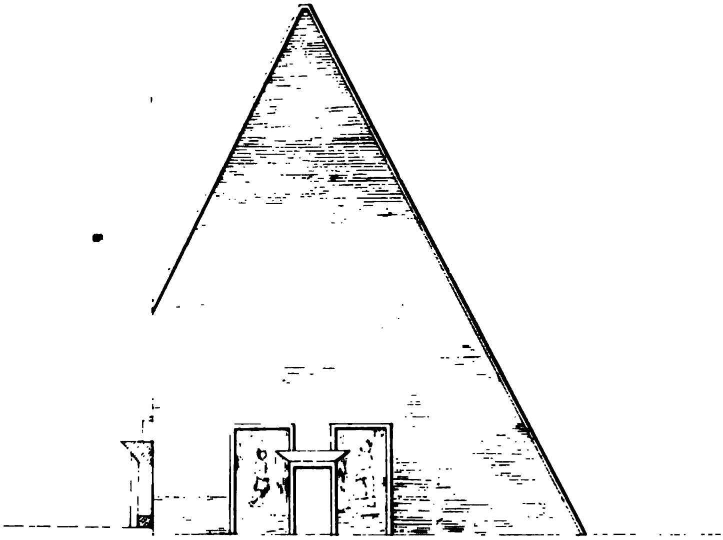


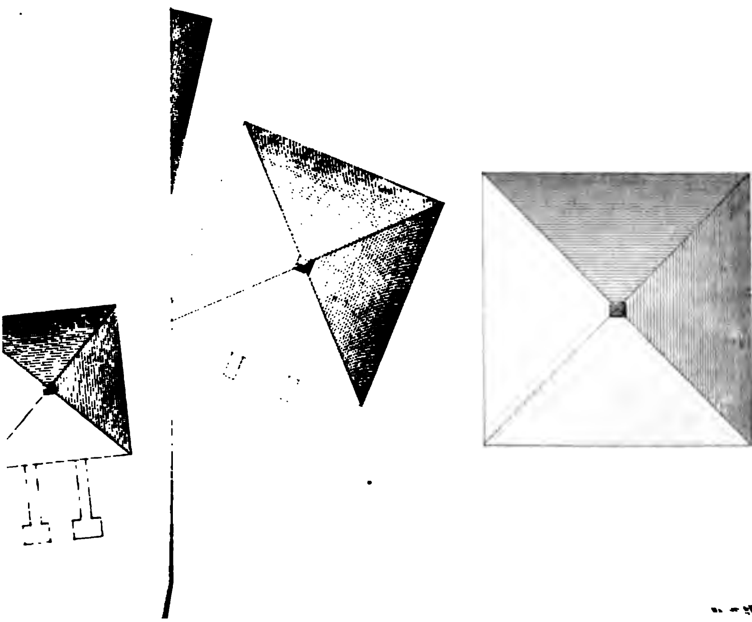
PYRAMID OF OAJACA TEHUANTEPEC.

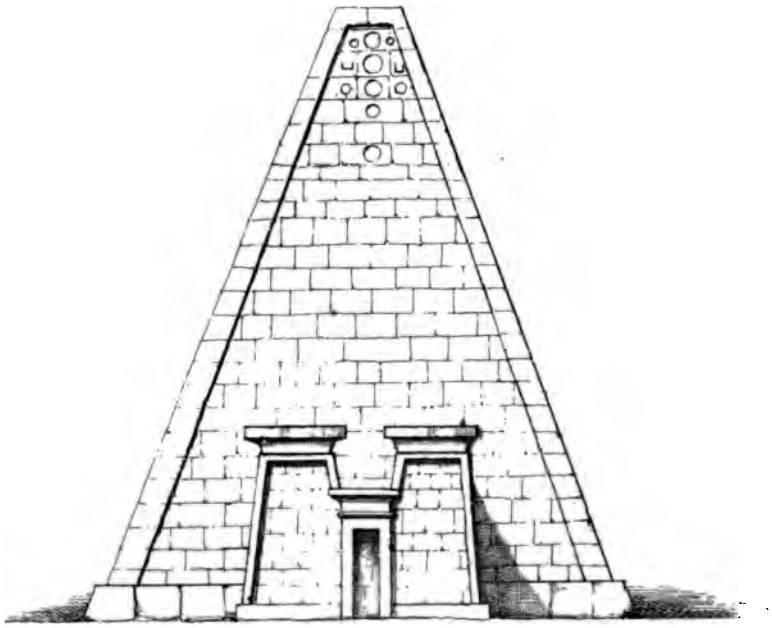


ELEVATION OF TEOCALLI AT PALENQUE.

SCALE.
1½ Inch to 50 Feet.

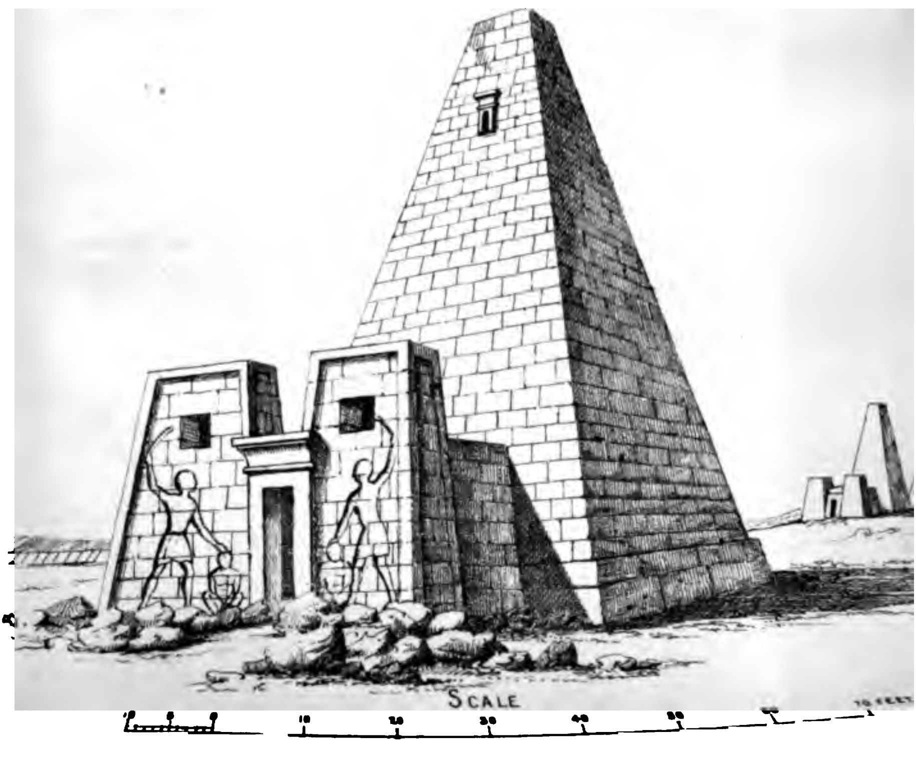






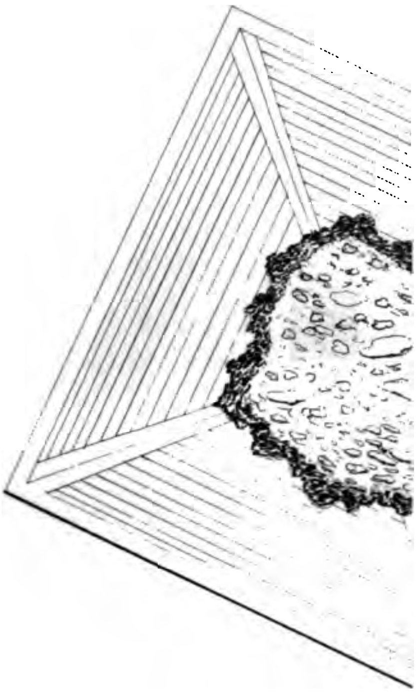
VARIETIES OF MEROË PYRAMIDS FROM LEPSIUS.

EXTREME CASES FOR LARGE SIZE OF PORCHES.



PLAN WITH TOP OF PYRAMI

SHEWING LOOSE MATERIAL INSIDE, ALSO THE



SCALE FOR FIG.

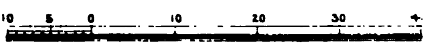
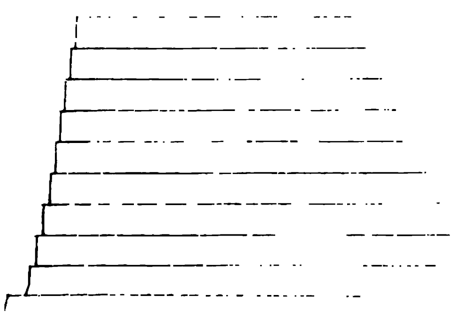
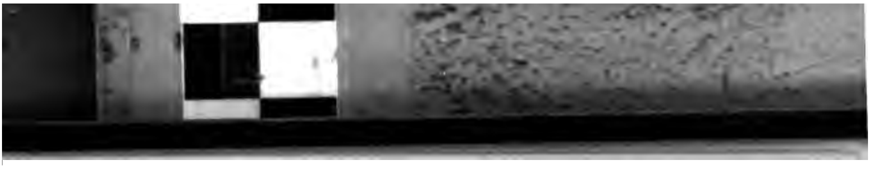
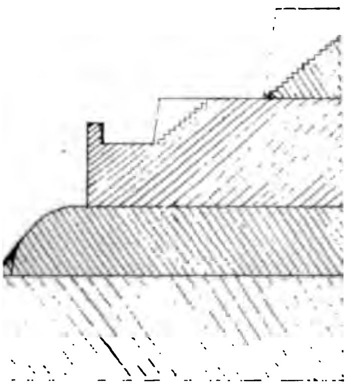


FIG 4.

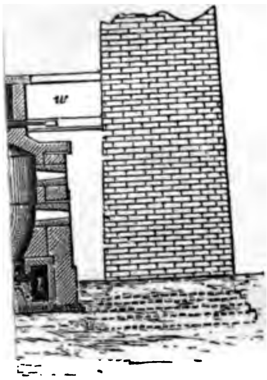




OR O



VERTICAL SECTION T



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